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THE THEORY OF EMOTION.

(I.) EMOTIONAL ATTITUDES.

BY PROFESSOR JOHN DEWEY,
University of Chicago.

In the following pages I propose, assuming Darwin's principles as to the explanation of emotional attitudes, and the James-Lange theory of the nature of emotion, to bring these two into some organic connection with each other, indicating the modifications of statement demanded by such connection. This close dependence upon results already reached, together with the impossibility of an adequate discussion of all details in the given limits (to say nothing of the immediate availability of most of the details in every one's experience), must be my justification for the generic, and even schematic, quality of the discussion. This may be regarded either as a sketch-map of a field previously surveyed, or as a possible outline for future filling in, not as a proved and finished account.

The necessity of bringing the two theories together may be seen from the fact that the very phrase 'expression of emotion,' as well as Darwin's method of stating the matter, begs the question of the relation of emotion to organic peripheral action, in that it assumes the former as prior and the latter as secondary.

1. Now this assumption, upon the basis of the discharge theory (as I shall call the James-Lange theory), is false. If one accept the latter theory, it is incumbent upon him to find the proper method of restating Darwin's principles, since there is no doubt of their substantial significance, however erroneous

may be their underlying assumption as to the relation of emotion and peripheral disturbance.*

Professor James himself does not seem to me to have adequately realized the inconsistency of Darwin's principles, as the latter states them, with his own theory; or the needed restatement would already have been performed by a much more competent hand than my own. At least he quotes, with apparent approval, explanations from Darwin which assume the priority of an emotion of distress to the contraction of the brows; and even suggests that Darwin does not go far enough in recognizing the principle of reacting similarly to analogous feeling stimuli.† Surely if James's conception of the origin of emotion is true, the statement that we react similarly to stimuli which feel alike must be translated into the statement that activities which involve, in like fashion, the same peripheral structures feel alike.‡

2. One does not, however, need to be committed to James's theory to feel the need of a different way of stating the particular undoubted facts discovered by Darwin. Physiologists

* While Darwin's language is that of the dependence of 'expression' upon emotion, it is interesting to note that so careful an observer has, in one place, anticipated and definitely stated the discharge theory, Expression of Emotions, p. 239. (My references are to the American edition.) "Most of our emotions are so closely connected with their expression that they hardly exist if the body remains passive —the nature of the expression depending in chief part on the nature of the actions which have been habitually performed under this particular state of mind." (Note in this latter phrase the assumption of the priority of emotion; but the continuation is unambiguous in the other sense.) "A man, for instance, may know that his life is in extremest peril, and may strongly desire to save it; yet as Louis XVI. said when surrounded by a fierce mob, "Am I afraid? Feel my pulse." So a man may intensely hate another, *but until his bodily frame is affected* he cannot be said to be enraged." (Italics mine.)

† Psychology, vol. II., pp. 480–81. The exactness of the latter statement may be doubted, as Darwin recognizes the facts, but includes them under the principle of serviceable associated habits (Expression, 256), as he certainly has a right to; for Mr. James himself recognizes (p. 481, footnote) that the 'analogous feeling' principle goes back to the teleology of the movements concerned.

‡ The *facts* conveyed in this principle seem to me of themselves a strong argument for the discharge theory. Left as Darwin and Wundt state it, all mediating machinery, physiological and psychological, is absent, and we cannot even start a hypothesis as to *how* a feeling (recognizing that it feels *like* another feeling!) sets out along the same afferent paths. Upon the discharge theory the mystery vanishes and we have the practical tautology: like affections of like structures give like feeling, the interest lying in the genetic tracing of the details.

agree that there are no muscles intended primarily for purposes of expression. A psychological translation of this would be that there is no such thing (from the standpoint of the one having the experience) as expression. We call it expression when looking at it from the standpoint of an observer—whether a spectator or the person himself as scientifically reflecting upon his movements, or æsthetically enjoying them. The very word 'expression' names the facts not as they are, but in their second intention.* To an onlooker my angry movements are expressions—signs, indications; but surely not to me. To rate such movements as primarily expressive is to fall into the psychologist's fallacy: it is to confuse the standpoint of the observer and explainer with that of the fact observed. Movements *are*, as matter of fact, expressive, but they are also a great many other things. In themselves they are movements, acts, and must be treated as such if psychology is to take hold of them right end up.

3. I shall attempt to show, hereafter, that this standpoint of expression of pre-existent emotion complicates and aborts the explanation of the relevant facts in the cases of 'antithesis' and 'direct nervous discharge.' At this stage I wish to point out that in the case of 'serviceable associated habits,' the principle of explanation *actually* used, whatever the form of words employed, is that of survival, in the form of attitudes, of acts originally useful not *qua* expressing emotion, but *qua* acts—as serving life. In the discussion of movements in animals (pp. 42–48) the reference to emotion is not even nominal. It is a matter of 'satisfaction of desire' and 'relieving disagreeable sensations'—practical ends. The expressions of grief and of anxiety (Chs. VI and VII) are explained, in their detail, whatever the general phraseology employed, by reference to acts useful in themselves. It would take up too much space to follow all cases in particular, but the book is open and the reader may easily discover whether in every case the idea of

* This, of course, is in no way inconsistent with the development of certain movements to serve as expressive. On the contrary, since movements take place in a social medium, and their recognition and interpretation by others is a fact of positive import in the struggle for existence, we might expect the development of gesture and signs through selection.

expression of emotion does not enter in only to confuse. *The reference to emotion in explaining the attitude is wholly irrelevant; the attitude of emotion is explained positively by reference to useful movements.*

An examination of one apparent exception may serve to clear up the principle. Of laughter, Mr. Darwin says, "We can see in a vague manner how the utterance of sounds of some sort would naturally become associated with a pleasurable state of mind" (p. 207). But Darwin does not use this idea, even in a 'vague' way. With his inevitable candor he goes on, "But why the sounds which man utters when he is pleased have the peculiar reiterated character of laughter we do not know."

Now I am not so rash as to attempt to deal in detail with laughter and its concomitant features, but I think something at least a little less vague than Mr. Darwin's account may be given. I cannot see, even in the vaguest way, why pleasure *qua* feeling (emotion?) should express itself in uttering sounds. As matter of fact it does not, nor even in smiles;* it is pleasure of a certain qualitative excitement or vivacity which breaks out in laughter, and what we can see, in a 'vague way,' is why excitement affecting the entire organism should discharge in the vocal apparatus. The problem is the discovery of that special form of excited action which differentiates the laugh from other excitations. Observe a crowd of amateurs just from a game. Note how, irrespective of what they say, you can judge whether they have won or lost. In one case postures are erect, lungs frequently expanded, movements quick, abrupt, and determined; there is much gesturing, talking, and laughing in high keys,—a scene which, looking at it 'ejectively,' we term one of liveliness, exhilaration, etc. In the other case there is little speaking, and that subdued; all movements tend to be slow, or, if rapid, indicate a desire to escape or expel something; meditative postures are frequently observed, etc., —a scene of depression. It is the contrast between spontaneous overflow and lowering of overt activity.

* The 'pleasures' of eating have their characteristic attitude—smacking lips, rolling tongue; the pleasures of sex theirs, etc. Many pleasures are accompanied by holding the breath to maintain the excitation at its maximum, not at all by the expiration found in laughter.

What is the difference? In either case the energy, muscular, nervous and visceral, aroused in the game, persists to some extent. What determines the antithetical lines of discharge of this surplus energy (that antithesis of 'dejection' and 'elation' running through all our terms)? In one case, I answer, there are frictionless lines of action, harmonized activity; or, in more psychological language, all existing kinæsthetic images reinforce and expand one another; in the other case there are two more or less opposed lines of activity going on —the images of the present situation and those of the past game cannot be co-ordinated. The energy is largely directed 'inwards'; that is, it is used up in rethinking the game, in making hypothetical changes, in recalling blunders (that is, images which one wants to expel), etc. The movements appropriate to the present activity cannot be identified with the nervous and motor energies which image the game. In the case of exhilaration, etc., there is identification of the thoughts (the nerve and muscular activities relative to the past game) and the present motor discharges.

The connection between *il penseroso* and melancholy more or less mild, and between *l'allegro* and joy, is thus organic and literal, not one of chance or analogy—as if analogy were somehow a force! When one can put up with his defeat, it ceases to bother him, he does not consider it longer. That is, the 'downcast' emotion and the intellectual reflection vanish together—the moment there is identification of images. The essential identity of the attitudes of thought and of regret is because of the condition of divided activity; there is still a struggle. Means and end are apart. The identity of attitudes of joy and of activity, of life (alert, wide awake, brisk, animated, vivacious, cheerful, gay—showy, lively, sprightly) is because of the unification of activity. Meditation and regret are both activities of arrest, of conflict; joy and 'lively' movement, of stimulation—expansion. No wonder, then, they have the same signs.

Thinking, to be sure, in certain professions, though not for the ordinary man, is an end in itself. In so far as thinking is an end in itself, the activity is unified and has its own joys. It ceases to be occupied with merely instrumental, and (therefore)

more or less burdensome, movements. Yet the pangs, the travail of thought, the arduousness of reflection, the loneliness of meditation, the heaviness of deliberation, are all proverbial. Only in rare cases is the whole system involved or unified, and the joy voluminous. Its ordinary form is the 'thrill' of identification or the satisfaction of 'good taste' in a clear, neat discrimination. When a long and comprehensive process is concluding and approaching its final successful or unified discharge, then, indeed, the hand of a Newton may tremble and joy become intoxicating. But I cannot admit, even in a half-hearted way, the idea that the sense of abundance and ease in thought (James, II. 477) may be purely cerebral.* It appears to me that it is in a literal sense that the object 'sets trains going'—these are revivals of motor discharge and organic reinforcement. Upon such occasions thinking becomes really whole-hearted; it takes possession of us altogether, and passes over into the æsthetic.

This, however, is only preparatory to the question of the specific 'sign' of joy, the laugh. How is that to be brought under this principle of being an actual portion of a useful activity? Why should the excitation, admitting that it affects the vocal organs, manifest itself in this form? While I feel pretty sure of the following explanation, I cannot hope that it will convince many. Though the result of considerable observation, it can be briefly summed up. The laugh is by no means to be viewed from the standpoint of humor; its connection with humor is secondary. It marks the ending (that is, the attainment of a unity) of a period of suspense, or expectation, an ending which is sharp and sudden. Rhythmical activities, as peek-a-boo, call out a laugh at every culmination of the transition, in an infant. A child of from one and a half to two years uses the laugh as a sign of assent; it is his emphatic 'I do' or 'yes' to any suggested idea to which he agrees or which suddenly meets his expectations.

A very moderate degree of observation of adults will con-

* Such distinctions as James makes here—in reality purely verbally—between spiritual and physiological, instead of between cerebral and viscero-motor, are what give the opponent the sole reason for labelling the theory materialistic—as if the bowels were really more material than the brain!

vince one that a large amount of laughter is wholly irrelevant to any joke or witticism whatever. It is a constant and repeated 'sign' of attaining suddenly to a point. Now all expectancy, waiting, suspended effort, etc., is accompanied, for obvious teleological reasons, with taking in and holding a full breath, and the maintenance of the whole muscular system in a state of considerable tension. It is a divided activity, part of the kinæsthetic images being fixed upon the immediately present conditions, part upon the expected end. Now let the end suddenly 'break,' 'dawn,' let one see the 'point' and this energy discharges—the getting the point is the unity, the discharge. This sudden relaxation of strain, so far as occurring through the medium of the breathing and vocal apparatus, is laughter. Its rhythmical character seems to be simply a phase of the general teleological principle that all well-arranged or economical action is rhythmical.* The laugh is thus a phenomenon of the same general kind as the sigh of relief. The difference is that the latter occurs when the interest is in the *process*, and when the idea of labor, slow and continuous, is at its height; while the laugh occurs when the interest is all in the outcome, the result—the sudden, abrupt appearance of the 'point.' In one case the effort is continued until it accomplishes something; in the other case the effort is arrested, and then the energy accumulated is set free from a seemingly outside source. The connection of humor with the laugh, and the ideas of relative superiority—triviality, and of incongruity, involved in humor, etc., seem to be simply more complex, and more intellectually loaded, differentiations of this general principle.

Not only are joy and grief practically in a peculiar qualitative antithesis, seeming to imply a common principle of which they are the extremes, but the 'signs' of joy and grief, especially when these become violent, are identical. This fact, otherwise so meaningless, becomes natural if we adopt the above explanation. Both crying and laughing fall under the same principle of action—the termination of a period of effort. If we fix our attention upon the conventional and

* Acute crying, etc., is non-rhythmical; when it does take the form of rhythmical sobbing, one experiences a sensation of relief—grief has 'moderated.'

literary conceptions of grief, this will seem far-fetched; if we take children and simple cases, it seems to stare us in the eyes. Crying is either a part of an effort to expel an intruder,* an effort so general as to engage spasmodically the lungs and vocal organs (a sort of general gripe); or, as we see so often in children, an explosion of energy, accumulated in preparation for some act, suddenly discharged *in vacuo* upon the missing of the essential part, the finishing factor of the act.†

Beginning with the simpler case, the phenomena of matured grief become easily explainable. They are phenomena of *loss*. Reactions surge forth to some stimulus, or phase of a situation; the object appropriate to most of these, the factor necessary to co-ordinate all the rising discharges, is gone; and hence they interfere with one another—the expectation, or kinæsthetic image, is thrown back upon itself.

4. In dealing with grief we have unconsciously entered upon a new field. The point of our third head is that the principle which Darwin calls that of 'movements useful in expressing an emotion' explains the relevant facts only when changed to read 'useful as parts of an act which is useful as movement.' In dealing with grief we have passed over into the phenomena of the breakdown of a given teleological co-ordination, and the performance of acts which, therefore, objectively viewed, are not only useless but may be harmful. My proposition at this point is that the phenomena referred to the principle of direct nervous discharge (the response to an idiopathic stimulus) are cases of the failure of habitual teleological machinery, through some disturbance in one or more of the adjusted members of the habit.

In order to avoid misconception, let me point out a great

* While Darwin's explanation of shutting the eyes—to protect blood-vessels from gorging on account of the violent screaming—undoubtedly accounts for the selection of this attitude, it can hardly account for its origin. I think originally it had the same end as screaming—to shut out or off some threatening object, as the ostrich, etc., or as one shuts his eyes on firing a gun the first time.

† I suppose every one has seen a young child go into a rage of screams and violent movements upon being handed, say, a broken cooky. The thing explains itself on the above principle. The concluding factor in a co-ordination of energy does not appear, and the child goes literally to pieces. I should like to see any explanation upon the anti-James theory, save that offered by Saint Augustine for similar phenomena of his infancy—total depravity.

ambiguity in the use of the term idiopathic. In one sense even the 'associated useful' movements are idiopathic, provided, that is, they originally were useful in reaching an end, and not simply in expressing an emotion. They are the reactions to their appropriate stimuli, and the sole difference between them and the liver changes, nausea, palpitation of heart, etc., usually classed as idiopathic, is that in them stimuli and reaction are more definitely limited to certain particular channels than in the latter cases; there is a defined, as against a vague and diffuse, direct nervous discharge. The fact that this defined discharge happens to be useful may state the kind of idiopathic response we have, but cannot make it other than a response. Furthermore, upon evolutionary principles, the limited, adjusted, and useful discharge must be a differentiation, selected and perpetuated because of its utility in the struggle for life, out of an original more diffuse and irradiating wave of discharge.

Admitting, then, that all emotional attitudes whatever are idiopathic in the broad sense, the sole difference being in the definiteness or limitation of the stimulus and its response, what are we to do with the cases now disposed of as 'idiopathic' in the narrower sense?—such phenomena as Mr. James briefly but excellently sums up on p. 482. My proposition, I repeat, is that all such idiopathic discharges, possessing emotional quality, are in reality disturbances, defects, or alienations of the *adjusted* movements. While not immediately teleological in the sense that they themselves are useful, they are teleologically conditioned. They are cases of the disintegration of associations (co-ordinations) which are serviceable, or are the use of means under circumstances in which they are totally inappropriate.

Idiopathic discharges which are not themselves adjusted movements or the disturbances of such adjusted movements do not appear to me to have any *emotional* quality at all. The trembling with cold or sheer fatigue is certainly qualitatively different from the tremble of rage or fear. The sensations of weakness in the bowels and of nausea, which are idiopathic to their appropriate stimuli, can be called emotional only by such a stretch of the term as renders all sensations and

impulses emotions, Professor James seems to me wholly successful in dealing with the charge brought that, upon his theory, all laughing ought to give the mirthful emotion, all vomiting that of disgust, etc.* The diffusive wave in one case is incomplete; but is there no reason or meaning in this difference? There is no doubt, in my own mind, that, *under existing conditions*, the supplying of the missing organic excitations will change the laugh and the nausea into mirth and disgust as emotions—this without any change in the 'object.' But whence and why these 'existing conditions'? The change from mere cachinnation to mirthful emotion is a distinct change in psychical quality, and this change of quality does not seem to be adequately *accounted* for by mere addition of more discharges—though, I repeat, simply adding on more discharges will undoubtedly *make* this difference. If these supplementary factors report the meaning or value of past co-ordinations, this change of quality is reasonable and inevitable; if not, if they are simply some more accidental discharges, the peculiar qualitative 'feel' is miraculous—it admits of no explanation.

This is but to say, from the psychological side, that all normal emotion of terror has an *object*, and involves an attitude *towards* that object; this attitude, under the given circumstances, perhaps not being useful, nay, being harmful, but yet the reproduction of an attitude or, rather, a mixture of attitudes which have been useful in the past. The uselessness of the attitude is due to the fact that some feature in the stimulus (the situation or object) awakens its appropriate reactions, but these do not co-ordinate with the reactions aroused by other features of the situation. The pathological emotion is, as Mr. James calls it, the *objectless* emotion, but its content is controlled by the active attitudes previously assumed towards objects, and, *from its own standpoint*, it is not objectless; it goes on at once to supply itself with an object, with a rational excuse for being.† This immediate correlation of the emotion with an 'object,' and its immediate tendency to assume the 'object'

* PSYCHOLOGICAL REVIEW, No. 5, p. 522.

† The pathological emotion is to the normal as hallucination is to perception. An unusual stimulus takes advantage of and controls the lines of co-ordination and discharge which have been built up with reference to the usual or normal stimulus. Psychologically the process is quite regular; it is only in its teleology that it is 'off.'

when it is not there, seem to me mere tautology for saying that the emotional attitude is normally rational in content (i. e., adjusted to some end), and, even in pathological cases, sufficiently teleological in form to subsume an object for itself.

In any case, upon James's theory, the admission of any idiopathic cases which cannot be reduced to abnormal use of teleological adjustments is more or less intolerable. Their permanent resistance to such reduction would be a strong objection to the theory. Hope, fear, delight, sorrow, terror, love, are too important and too relevant in our lives to be in the main* the 'feel' of bodily attitudes which have themselves no meaning. If the attitude is wholly accidental, then the emotion itself is brute and insignificant, upon a theory which holds that the emotion is the 'feel' of such an attitude.

One more word of general explanation. The antithesis here is between the merely accidental and the adjusted excitation—not between the mechanical and the teleological. I add this because of the following sentence in James: "It seems as if even the changes of blood-pressure and heart-beat during emotional excitement might, instead of being teleologically determined, prove to be purely mechanical or physiological outpourings through the easiest drainage-channels" (II. p. 482). Certainly, if these are the alternatives, I should go a step farther and say that even the clenching of the fist and the retraction of the lips in anger are simply mechanical outpourings through the easiest available channel. But these are not the alternatives. The real question is simply how this particular channel came to be the easiest possible, whether purely accidentally or because of the performance of movements having some value for life preservation. The ground taken here is that the easiest path is determined by habits which, upon the whole, were evolved as useful.†

*In the main, I say; for doubtless it is pedantry to hold that every slight feature of the attitude is conditioned by an activity directed towards an object.

†It is admitted, of course, as Mr. James puts it, that there are "reactions incidental to others evolved for utility's sake, but which would never have been evolved independently" (p. 484). Indeed, in one sense of the term 'incidental' this is a necessary part of my proposition. The only question is whether 'incidental' means purposeless, or means having their purpose not in themselves, but as relative to, as facilitating or reinforcing, some other useful act. The fact, once more,

Coming a little more to details, it is obvious that the teleological principle carries within itself a certain limitation. Normal and usual are identical; the habit is based upon the customary features of the situation. The very meaning of habit is limitation to a certain average range of fluctuation. Now if an entirely strange (forgive the contradiction in terms) stimulus occurs, there will be no disturbance of function, though the organism may be destroyed by the impact of the foreign force. But let some of the features of a situation habitually associated in the past with other features be present while these others fail, or let the ordinary proportion or relative strength of stimuli be changed, or let their mode of connection be reversed, and there is bound to be a disturbance and a resulting activity which, *objectively viewed*, is non-teleological. We thus get an *a priori* canon, as it were, for determining when, in a given emotion, we shall get symptoms falling under the 'serviceable associated habit' principle and when under the idiopathic. Whenever the various factors of the act, muscular movement, nutritive, respiratory, and circulatory changes, are co-ordinated and reinforce each other, it is the former; whenever they interfere (the 'idiopathic'), the 'feel' of this interference *is* (applying the general principle of James) the pathological rage, or terror, or expectation.

Once more, we work in a wrong, a hopeless direction when we start from the emotion and attempt to derive the movements as its expression; while the situation clears itself up when we start from the character of the movement, as a completed or disturbed co-ordination, and then derive the corresponding types of normal and pathological emotion. We can understand why the so-called idiopathic principle comes into play in all cases of extreme emotion, the maximum limit seeming to be the passage into spasm when it assumes a rigid type, of hysteria when it involves complete breakdown of co-ordination.

The attitude of normal fear may be accounted for upon direct teleological principles; the holding of breath marks

that upon Darwin's method of statement no such relative or incidental movements can be admitted is an undoubted objection to Darwin's mode of statement of the principle of useful habit.

the effort; the opening of mouth, the act arrested half-way; the opening of eyes, the strained attention; the shiver, of retraction; the crouching down, the beginning of escape; the rapid beating of heart, the working up of energy for escape, etc. Now if these activities go on to complete themselves, if, that is, they suggest the further reaction which will co-ordinate into a definite response, we get judicious fear—that is, caution. Now if these do not suggest a further movement which completes the act, some or all of these factors begin to assert themselves in consciousness, isolatedly or in alternation—there is confusion. Moreover, each particular phase of the act which is normal in co-ordination, as the more rapid beating of the heart, being now uncontrolled by lack of its relevant motor associates, is exaggerated and becomes more and more violent. The response to the normal demand for more nutrition finds no regular outlet in supplying the motor-energy for the useful act, and the disturbances of viscera and associated organs propagate themselves. The trembling marks, so far as I can see, simply this same disco-ordination on the side of the muscular system. It is the extreme of vacillating indecision; we start to do this, that, and the other thing, but each act falls athwart its predecessor.

Speaking roughly, there is exaggeration of the entire vegetative functions of the activity, and defect of the motor side—the unstriped muscles being included, on a functional basis, with the vegetative system. Now this is just what we might expect when there is a great stirring up of energy preparatory to activity, but no defined channel of discharge. Thus the agent becomes entirely taken up with its own state and is unable to attend to the object.

The pathological emotion is, then, simply a case of morbid self-consciousness. Those factors of the organism which relate most immediately to the welfare of the organism, the vegetative functions, absorb consciousness, instead of being, as they normally are, subsidiary to the direction of muscular activity with reference to the 'object.' This is equally true in extreme terror, and in being 'beside one's self' with anger. The cases in which sanguine excitement and apprehension affect the bladder will be found, I believe, to be almost uni-

formly cases where it is not possible to do anything at once with the aroused activities; they cannot be controlled by being directed towards the putting forth of effort upon the 'object,' that being too remote or uncertain.

Certainly, the principle for attitudes commonly called those of morbid self-consciousness is precisely the one just laid down. In these cases muscular (not vegetative) functions normally useful in the attainment of an end are first aroused in response to stimuli, and then, not being completely co-ordinated into action, are *not* used with reference to the end, and so stand out in consciousness on their own account. I shall not attempt any detailed statement here, but leave it to the reader to answer if the above does not give a precise generic description of the sensations of awkwardness, of bashfulness, of being ridiculous (as when one starts an appropriate movement, but is made conscious of it in itself apart from its end) on one side, and of affected grace, mincing ease, pomposity and conceit on the other.

All these facts taken cumulatively seem to me to render it fairly certain that the 'idiopathic' cases, as a rule, are to be conceived of as the starting of activities formerly useful for a given end, but which now, for some reason, fail to function, and therefore stand out in consciousness apart from the needed end.

5. I come now to the principle of antithesis. According to Mr. Darwin, when certain movements have been habitually of service in connection with certain emotions, there is a tendency, when a directly opposite state of mind is induced, to the performance of movements of a directly opposite nature, '*though these have never been of any use*' (p. 50, italics mine). Here we have a crucial case; if the antithesis of the emotion determines the antithesis of expression, James's theory is, in so far, overthrown; if, on the other hand, the antithesis of 'expression' goes back to activities having their own ends, the ground is at least cleared for the discharge theory.

Beginning with animals, Mr. Darwin illustrates his principle of antithesis from the cat and dog. No one can read his account or examine the pictures without being convinced that the movements *are* antithetical. But there is something

intolerable to the psychologist in the supposition that an opposite emotion can somehow select for itself channels of discharge not already used for some specific end, and those channels such as give rise to directly opposed movements. Antithesis is made a causal force. Such an idea is not conceivable without some presiding genius who opens valves and pulls strings. The absence of mediating machinery, of interlinking phenomena, is even more striking in this case than in that of 'analogous feeling.'

If, again, the matter be treated as a case of the connection of movements with reference to certain acts, the mystery vanishes. Mr. Darwin's cases are taken from domestic animals. Now wild animals have, speaking roughly, just two fundamental characteristic attitudes—those connected with getting food, including attack upon enemies, and those of defence, including flight, etc. A domestic animal, by the very fact that it is domestic, has another characteristic attitude, that of reception—the attitude of complete adaptation to something outside itself. This attitude is constituted, of course, by a certain co-ordination of movements; and these are antithetical to those movements involved in the contrary attitude, that of resistance or opposition. A study of the dogs upon pp. 52–55 will show that the attitude of opposition is naturally self-centred and braced, the best position from which to fall, on one side, into an attitude of overt attack, and, upon the other side, into that of resistance to attack. The attitude of 'humility' and 'affection' consists, as Mr. Darwin well says, in continuous, flexuous movements. These movements are precisely those of response and adaptation. The centre of gravity is, as it were, in the master, and the lithe and sinuous movement is the solution of the problem of maintaining balance with respect to every change in this external centre of gravity. It is the attitude of receiving favor and food from another. The dependence is actual, not symbolic. Unless Mr. Darwin were prepared to equip the animal with a full-fledged moral consciousness, the 'humble' attitude of the dog can hardly be other than the habitual attitude of reception, or the 'affectionate attitude other than the recurrence of movements associated with the food-getting. The same general principle will apply

to the antithetical cat expressions, save that the dependence in the case of the cat assumes more the form of passive contact and less that of active adjustment. The reminiscence of sexual attitude is possibly also more marked.*

The other cases of antithesis given by Mr. Darwin are the shrug of impotence, and the raising of the hands in great astonishment. I feel certain that the rational hypothesis is to suppose that these are survivals of certain acts, and not symbolic indications of certain emotions. As a contribution to such a working hypothesis, I suggest the possibility that the throwing up of the arms in attention is partly the survival of a movement of warding off the approaching hostile object, and partly a reinforcement of the holding of the chest full of air characteristic of expectancy and of astonishment—a movement whose analogue is found in the raising and drawing back of the arms in yawning.† The shrug of impotence seems to be complex; the union of survivals of three or four distinct acts. The raising of the brows is the act of retrospect, of surveying the ground to see if anything else could have been done; the pursing of the lips, the element of tentative rejection (doubt); the raising of the shoulders, the act of throwing a burden off (cf. 'he shouldered it off on some one else'); the holding out of the hand, palm up, the attitude of asking or taking. To my introspection the *quale* of the emotion agrees entirely; it is a feeling of 'I don't see how I could possibly have done anything else, so far as I am concerned, but I'm willing to hear what you have to offer'—of 'I don't know; you tell.' It thus has the distinctly expressive or social element in it, and marks the passing over of emotional attitude into gesture.

Summing up, we may say that all so-called expressions of

* Being unable to do anything with these cases, I called them to the notice of my friend and colleague, Mr. G. H. Mead. The explanation given, which seems to me indubitable, is his. The relation between the vegetative and the motor functions, given above in discussion of pathological emotion and to be used again below, I also owe to him. While I have employed the point only incidentally, Mr. Mead rightly makes it essential to the explanation of emotion and its attitudes, as distinct from the identification and description which alone I have attempted. I hope, therefore, that his whole theory may soon appear in print.

† Since writing the text, I have repeatedly noticed this attitude of the arms, without the rigidity, assumed by a child of two years while watching the preparation of his food.

emotions are, in reality, the reduction of movements and stimulations originally useful into attitudes. But we note a difference in the form and nature of the reduction, and in the resulting attitudes, which explain the apparent diversity of the four principles of 'serviceable associated habits,' of 'analogous stimuli,' of 'antithesis,' and of 'direct nervous discharge.' A given movement or set of movements may be useful either as preparatory to, as leading up to, another set of acts, or in themselves as accomplished ends. Movements of effort, of bracing, of reaching, etc., evidently come under the former head. Here we have the case of useful associated movements in its strict sense. The culmination of all these preparatory adjustments is the attainment of food or of the sexual embrace. In so far as we have attitudes which reflect these acts, satisfying in themselves, we get cases of so-called analogous stimuli. The antithetical attitudes of joy and grief, and all that is differentiated from them, mark the further development of actual attainment of an end, (or failure to get it) occurring when the activity specially appropriate to the particular end reached (or missed) is reinforced and expanded by a wide range of contributory muscular and visceral changes. The cases of failure bring us to the breakdown of co-ordinations habitually useful, to their alienation, or to reciprocal disturbance of their various factors, and thus to the facts usually subsumed under the idiopathic principle. In this progression we have a continually changing ratio of the vegetative to the motor functions. In the preparatory adjustments the latter has the highest exponent, and the strictly emotional *quale* of feeling is at its minimum. In joy and grief, as in less degree with 'sweetness,' disgust, etc., the organic resonance is at its height, but strictly subservient to the motor performances. In the idiopathic these vegetative functions break loose and run away, and thus, instead of reinforcing the efficiency of behavior, interfere by their absorption of consciousness.

In the following article I shall take up the discharge theory of the nature of emotion, and discuss it in the light of the conclusions now reached.

THE STUDY OF A CASE OF AMNESIA OR 'DOUBLE CONSCIOUSNESS.'

BY PROFESSOR CHARLES L. DANA, M.D.,

Bellevue Hospital Medical College.

The following case of altered personality seems to me to have a special psychological interest, for the reason that it is so classical a type, and also because there are in it no disturbing elements of hysteria, epilepsy, or organic disease.

In 1884 I collected and published all the recorded cases of 'double consciousness' or 'periodical amnesia' then accessible (Wood's Reference Handbook, art. 'Disorders of Consciousness.') They numbered at that time sixteen. Since then several new cases have been reported by Guinon, Janet, James, Weir Mitchell, Baker, and others. The subject has been discussed at length by Ribot (Diseases of Memory) and by Prof. Wm. James (Psychology, vol. II.), also recently by Alfred Binet (Internat. Scient. Series, 1892), and Pierre Janet (Rev. gén. des Sc., Mar. 1893). Most of the reported cases are evidently illustrations of epilepsy, hysteria, or a peculiar form of melancholia. In epileptic cases the amnesia is associated with automatism, and the patient's mental powers are sharply limited and lessened. The same is true, so far as my experience goes, in hysteria, the altered personality being only an illustration of an automatic or trance condition. Huxley's traumatic case is one of this kind, and they are not very rare in neurological experience. I must be permitted also to express grave doubts as to the value of psychological studies made upon the trained hypnotic patients of Salpêtrière. A personal knowledge of some cases of this class and a perusal of the investigations of Mr. Ernest Hart upon the subject justifies this view and a distrust of the multiple personalities of trained Parisian subjects.

I do not wish to disparage the reports of others, but I am sure a candid student of the cases of alleged double personality will find that what may be called pure cases are few. Such are those of Dr. Mitchell, Dr. Donar (Trans. Roy. Soc. Edin., 1822), Dr. J. N. MacCormack (*Medical Record*, May 26, 1883, p. 570), of M. Azam, and the present one.

History of the Case.—The patient, Mr. S., age 24, was an active, intelligent, and healthy young man. Though coming of a somewhat nervous stock, there is no actual psychosis in the family. He had himself always been well. His habits were good. For a year or two before his trouble came on he had been subjected to some nervous strain, but it had not perceptibly affected his health or spirits. About two weeks before his accident he had some financial trouble, and on coming home had a 'nervous chill.' However, he seemed perfectly well next day and continued his usual duties. On Friday evening, November 18th, he retired as usual. Next morning, as he did not appear at breakfast, a member of the family entered his room and found it full of gas, and the patient lying unconscious in bed. The escaping gas was due to a leak in the pipe, as was subsequently found. The stop-cock of the gas-burner was turned off, and there was no possible reason for or suspicion of suicide. The patient was as stated unconscious, the face livid, the lips blue, the eyes open, the respirations slow and stertorous, sometimes almost ceasing. The family physician, Dr. Rodenstein, was called, and worked over him for three hours before the breathing became natural and his life seemed out of danger. He became partially conscious by 4 P.M., and to a clergyman who had called he talked rationally but not clearly. Next morning he recognized his sister and father, and said he thought he was losing his mind. In the afternoon he became somewhat delirious. He slept that night, but during the succeeding six days his mind wandered and he was apparently distressed and excited. He was oppressed with the idea that some one wanted to take him away and do him a bodily injury. He talked about a trip he had been expecting to make to Washington, and called for his time-tables. He spoke also about his business and of various plans he had been intending to carry out. On Tuesday, four

days after the accident, he was seen trying to read a newspaper upside down. On the eighth day he was taken to Dr. Granger's sanitarium. He went without trouble, though he was still somewhat excited and maniacal. That night he slept and next morning awoke free from any signs of mania. He was quiet and sane in every way. From this time the evidences of his amnesia and changed personality were apparent. He dressed himself neatly and with his usual attention to his toilet, understanding apparently the use of the various articles of dress. He showed by his conversation at once that he did not know who he was or where he was, and that his conscious memory of everything connected with his past life was gone. His vocabulary at first was very limited; he could only use familiar words, and could only understand language of the simplest character, such as that bearing on the things immediately about him. He did not know the names or uses of the things in and about the house, though he at once remembered and never forgot any name told him. Consequently his vocabulary and understanding of conversation rapidly increased. He had a German attendant, and pronounced many of the new words with a German accent. Everything had to be explained to him, such as the qualities and uses of the horse and cow and of the various articles about the house. Yet he would sit at the table and eat his meals with his former neatness, preserving also the courtesies and amenities of a gentleman, but he could not understand why he did certain things until it was explained. He did not recognize his parents or sisters, or *fiancée* though he said that he had always known the latter, and his great desire and longing was to have her with him. He did not remember the slightest detail of his former relations with her and did not know what marriage meant or the significance of the filial relation. Those persons whom he had liked very much before he seemed especially glad to see, though he could not explain why.

He could not read, and did not even know his letters or figures. But he soon learned both to read and write simple sentences involving ordinary words.

His vocabulary was gradually increased, but even two months after his accident he could not read a newspaper un-

derstandingly, except simple accounts of every-day happenings. He was naturally slowest in understanding abstract terms. He learned figures and arithmetic very quickly and could soon do ordinary arithmetical computations easily. He had been accustomed to play billiards a little, but played the game badly. He very soon learned to play again, appreciating the value of angles, and before long he became much more skilful than he had been in his former state. He had always been clumsy with his hands and never liked mechanical work or showed the least capacity for it; he never could draw or carve. With a little instruction from another patient he soon became very skilful in carving and worked a monogram in the back of a brush in a most creditable manner. He also made a shuffleboard, doing the work very neatly. He showed in fine a much greater cleverness with the hands and finer development of muscle-sense than he had had before.

He used to play and sing a little. About six weeks after the accident he picked out a tune on the piano which he had known long before, but had not heard or played for a year. He did not know what it was, or associate it with any early memory. He sang some of his old songs and played a little on his banjo. The old musical memories were there, but dissociated from any thoughts of the past. He was very imitative and his memory for everything told him was extraordinarily retentive. He had always been careful and even fastidious about his person, and he continued to be so. His habits of courtesy and affability continued the same.

He had had some religious upheavals in the past. Two or three years before, he was distinctly and positively atheistical; later he was more inclined to theism and agnosticism. In an argument which I undertook with him to test his logical powers and knowledge of abstract ideas he showed a distinctly atheistical state of mind. His views were those held some years previously, not his later ones. In argument he showed considerable dialectic skill and logical power. But he evidently could not understand any conceptions at all abstract. His 'mind-stuff' was made up of conceptions closely related to his recently acquired practical knowledge. He had previously acquired a special repugnance to any form of

religion, and he showed this feeling of antagonism in his conversation.

He was even-tempered and obliging. He had never been demonstrably affectionate and was not in his new state, except as regards his *fiancée*, about whom his thoughts and feelings were intensely centred.

If one were to meet him, and discuss ordinary topics, he would show no evidence of being other than a normal man, except that he might betray some ignorance of the nature or uses of certain things. His conversation ran chiefly on the things he did every day and on the new things he every day learned. He was exactly like a person with an active brain set down into a new world, with everything to learn. The moon, the stars, the animals, his friends, all were mysteries which he impatiently hastened to solve. He was somewhat sensitive to his condition and did not like to meet persons whom he had known before. He cherished also a lurking suspicion that some one in some way might want to take his *fiancée* away from him. But he never was in a passion, never became incoherent or delirious, had no delusion or hallucination, and was not in the slightest degree demented.

He spoke of his own mental condition, and seemed to understand that it was not right. He was very anxious to get well.

Physically his health was perfectly good. He had no anæsthesia of the skin, no limitation of visual or aural fields, no stigmata of a trance or hysterical state. He slept well, and so far as I know had no dreams. He had a tendency to coldness and redness of the extremities, and there was evidently lack of vaso-motor tone. At times when a little excited he would move his head constantly from side to side as if working in an uncomfortable collar. This was a violent exaggeration of a habit I observed that he had when in his normal condition.

On three occasions I hypnotized him, using the methods of Braid and Bernheim combined. On the second and third trial I put him in a light degree of hypnotic sleep. During this I told him that after waking at a certain signal he would go through certain acts, such as rubbing his eyes, walking about the table, opening the door and giving a certain greeting

to his mother. Also that at a certain hour in the evening he would remember the past. He did everything that I suggested except the last. At the time named in the evening, he simply said without suggestion, "Dr. Dana told me to remember something, but I can't do it."

I saw him once or twice a week at my office. He continued in much the same state day after day. His knowledge increased so that he was able to go about alone to a considerable extent, and I had begun to advise his going to his old place of business and learning something of his old work.

At the suggestion of Prof. Josiah Royce, to whom I gave some account of the case, I told him to get some of his old love-letters and copy them, also to copy some of the prayers that he used to say daily as a boy, and finally to get some of his old business accounts and copy them off; I was in hopes that some of these things might revive old memories by appealing to his affections, his religion, and his business instincts. He did this, but with no apparent success.

On February 15th, Friday evening, exactly three months from the time of his attack, he went to see his *fiancée*. She thought, after the interview, that he was rather worse, less like himself. She cried that night when he left, thinking he would never get well. While riding home with his brother he said he felt as though one half of his head was prickling and numb, then the whole head, then he felt sleepy and was very quiet, but did not fall asleep. When he got home he became drowsy and was carried to bed, where he fell asleep. At about 11 o'clock he woke up and found his memory restored. He remembered distinctly the events of three months ago: his visit to his *fiancée*, his supper at the club afterwards, his journey home, his shutting his bedroom door and getting into bed. His memory stopped there. He did not recall a thing that had occurred between times.

He knew all his family at once and was plainly just the same man as before. But the three months was an entire blank to him. Next day he came to see me, but did not know me (I had never seen him before his accident). Not a thing connected with the three months could be recalled. It was so much taken entirely out of his existence. He at once resumed

his old work and habits, and has continued perfectly well up to the present time.

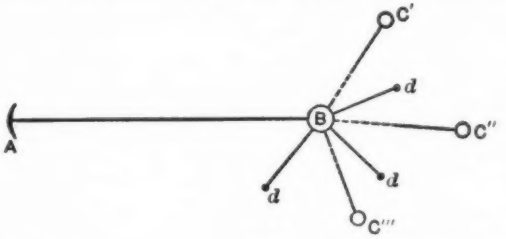
Remarks.—I do not pretend to psychological expertness, and my training has been such that I am obliged to formulate my psychological conceptions in physiological terms. A simple visual sensation is always represented to my mind by a nervous impulse travelling from the retina to a certain part of the occipital cortex. If this sensation is complex and arises to the dignity of the perception of, say, a man, a mass of association-fibres send out their impulses, awakening previously-stored images of man. If it is an old friend, the cortical areas for a still larger space are sent tingling into activity, and memories of old days rush into consciousness, intensifying it, awakening pleasurable feeling and suggesting new or reviving old ideas.

Now it was just this power of bringing into activity these longer association-fibres which was lost in the case of S. To him at first a man was a simple visual sensation, perhaps a little more, but not very much. Those associative tracts which formerly brought quickly into consciousness his previous visual, auditory, and tactile memories of man were paralyzed.

Yet certain shorter fibres were not altogether incapable of activity, and helped by subconscious memories they soon worked quickly and helpfully, enabling him to form again a fresh conception of the creature man. This idea of 'man,' however, was one almost entirely new and narrow. The idea of a father or teacher or old friend he did not in the least understand. The old and more complex memories lay quiescent in the mind; they could not be reached by any strands of connecting nerves that had been formerly so active. Certain memory-feelings, however, of affection, love, friendship, were associated with the new conception. He was glad to see and at once enjoyed the presence of an old friend, whom he had been intimate with, but about whom he could recall nothing. A previous special affection for mother, sisters, and others showed itself in a desire to be with them and enjoyment of their society.

I can only conclude that in this condition there was a paralysis or suspended function of those longer especially-

trained associative tracts leading to past memories. I can perhaps make my meaning clearer by a very crude diagram.



A = eye. *B* = cortical centre for vision.
C' C'' C''' = old memories acquired in past experiences.
ddd = new memories in active association with *B* and acquired since accident.
BC' BC'' BC''' = special association-fibres connecting visual centre with memory areas.

Can neuro-pathology throw any more light on this process?

There does occur at times a destruction of certain parts of the brain cortex as a result of which all memories of a certain class are abolished, leaving the intellect otherwise clear. A patient of mine after a slight right-sided stroke lost entirely the power of reading. She did not know even the letters that she saw. She could write fluently and coherently, but could not read a word of what she had written. In her case there was a softening of the left angular gyrus extending into the white matter and involving the association-tracts. Yet her word-memories of the past were not destroyed because by making her trace the letters with a pencil she could slowly read through her muscular sense; and on reading a sentence its meaning was clear to her. But she could never be taught to read with her eyes. Now as far as a loss of word-memory is concerned she was exactly like my patient who at first held his newspaper upside down. In her case the sense-centre *B* was partly destroyed, and the association-fibres with it, while the remoter areas which had previously been stored up by her reading were intact. In Mr. S. the centre *B* was present and soon excited to activity, but the remoter memory-areas could not be brought into activity by any form of sensory stimulation or excitation of newly-acquired or subconscious memories.

Besides, in Mr. S. what was true for vision was equally true for auditory memories. He did not at first understand any but the simplest language, nor appreciate the significance of sounds or gestures. Suppose I represent these various centres by *B*, *C*, *D*, *E*. Then if around each one I were to draw a dark circle it would represent the inactive association-tracts, which kept his centres when stimulated from arousing old memories. A further dead line would have to be assumed to exist for his muscular memories, his affections, and his automatic and subconscious activities. This necessitates, perhaps, a clumsy mechanical conception, but so far it appears to me to be absolutely correct from a physiological point of view.

Suddenly all these dead walls fall away, the associations are renewed, old memories and the normal personality are restored. It must be remembered that the dead walls are not absolutely solid, but that association-fibres, never before much used, pass through them, and that by their means new memories were quickly established, connections with older subconscious ones were in a measure made, and complex mental processes were possible.

Such a conception as I present excludes as impossible any theory of a dual brain or of one cerebrum being put to sleep except on the hypothesis that all memories are, when solidly established, located in the one cerebrum. But the facts of pathology disprove this.

I must assume, therefore, that the association-tracts which ordinarily connect sensory areas with long-stored-up memories are only put in action by a specialized and highly-differentiated power on the part of nerve-cells. And this is certainly the case. The simplest function of a receptive cell when excited is to awaken a simple sensation, next to arouse some other simple sensation-memories, so that a person sees and knows an orange. But to recall past experiences, gustatory, personal, etc., in connection with that object calls for a higher and complex function. Thus the nerve-cells have a special memory-arousing function.

This is often lost temporarily, as in states of abstraction, or trance states, excitement, epilepsy, delirium, insanity, and dementia. But to overthrow this special memory-function of

the cell for the whole brain requires a peculiar and exceptional kind of stimulus.

In fine, the hypothesis which I put forward, very tentatively, is that the nerve-cells have special and highly-developed functions which are in relation with special lines of association-fibres, and that this specialized function is suspended in such a case as that which I have described; just as special functions and memories are inhibited in hysteria.

If we come to consider this case from another point of view, viz., the strictly neurological, one could say that at the very first my patient had complete *apraxia*, including mind-blindness and word-blindness, mind-deafness and word-deafness, mind anosmia, ageustia and amimesis, in fact a total loss of memory for the significance and uses of things and of language. He gradually but with varying rapidity recovered from his word blindness and deafness so that he could read and understand spoken words. His apraxia, as regards the ordinary things of his daily life, soon disappeared so that he could dress, eat, and go about much as others. But a residuum of apraxia remained, and this bore upon abstract ideas, the facts of social and domestic relations. He did not understand the relationships of parent and child, husband and wife, the significance of business acts or the current events. In fact none of the memory-pictures stamped upon his brain by his life-experience previous to his injury could be revived.

The fact that the memory-disturbance in Mr. S. was due to some inhibitory action on a special function of the cells is supported by other clinical facts. It is not by any means rare for persons who receive a severe head injury to suffer from a temporary amnesia. In my own experience I have seen such cases. A youth was suddenly knocked down by a blow on the head. He was not made unconscious, but was slightly stunned. He was helped upon his feet and presented no marks of severe physical injury. But he did not in the least remember where he was, what his name was, where he lived, or what he had been doing. He talked coherently and understood questions, but there was a total amnesia of the past (retrograde amnesia of Charcot). His symptoms gradually improved, and in a few weeks he was entirely well.

It is an acknowledged physiological law that by a severe mechanical shock we can suspend the functions of cell-groups partly or entirely. So with regard to the brain it seems to me easy to believe that the memory-centres have special association-tracts by which they are kept in association with the cortical centres for the special sensations. These tracts must be represented by nerve-fibrils of especial tenuity and delicacy (*BC'*, etc.); it may even be that there is a special part of the cell or special cells with special lines of collaterals differentiated for this function and represented by the dotted lines in my diagram. If this associating function or process of 'revival' is indeed psychologically and physiologically a special one, then it has a definite anatomical representation. And one can understand how poisons and shocks suspend its function. The very fact, indeed, of the comparative frequency of milder forms of shock-amnesia confirms the view that there is such a unity as I suggest.

An interesting practical point exists in connection with this case. Rouillard states that carbonic-acid gas is particularly liable to cause defects in memory, and he even adds that children who are brought up in houses with open fires and insufficient draught are apt to have defective memories. (*Gazette des hôp.*, May 7, 1892.) Fallot has reported the case of a person who attempted suicide by charcoal-fumes. After recovery the memory was defective not only for events subsequent to the poisoning, but extending back to three days prior thereto (retrograde amnesia). (*La Semaine medicale*, Mar. 3, 1892.)

EXPERIMENTS IN SPACE PERCEPTION. (II.)

BY DR. JAMES H. HYSLOP,

Columbia College.

There is another experiment which throws farther light upon the question of magnitude and its perception as discussed in the previous article. It is illustrated in Fig. 4. Draw the circles *A* and *B* and *C* and *D* on separate cards, so that one of them can be held above the other and at a greater distance from the eyes. Also draw *C* and *D* farther from the median line or plane *MN*, so that when placed at a certain distance farther from the eyes than *A* and *B*, the planes *LY* and *OY* may pass respectively through the centres of *C* and *A* and *D* and *B*, and terminate in the retinas of the eyes after crossing, *E* being the point of fixation. This enables the same degree of convergence to combine *C* and *D* at the same time that *A* and *B* are combined. When this is done the effect is as follows. The central circle, which is the fusion of *C* and *D*, appears to be much smaller than that of *A* and *B*. Now as the retinal image cast by *C* and *D*, they being farther from the eyes, is smaller than that of *A* and *B* (the circles being of the same size), we might say that the phenomenon is due wholly to that fact. But as previous experiments have shown, if the centres of *C* and *D* are at the points *H* and *I* respectively, the central circle *does not* seem any smaller than the central image of *A* and *B*. Measurement shows the fact. This cannot be determined easily by introspection, because under the circumstances the fusion of *C* and *D* does not take place at the same time as that of *A* and *B*. But the comparison by memory, which is that of two successive seconds or moments, makes them appear the same. However, as this is exposed to error, we can rely upon the measurement mentioned above, and that, as we have seen, makes the central circle the same in magnitude for all degrees of convergence. Now if the dimin-

ished magnitude of *C* and *D* under convergence, when their

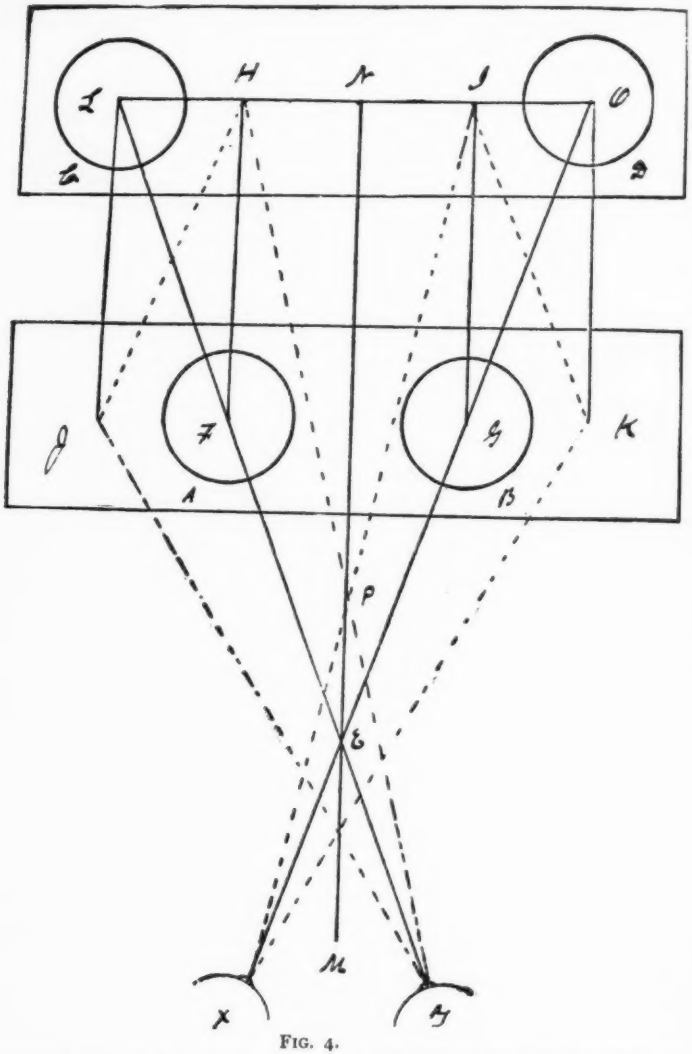


FIG. 4.

centres are at *L* and *O*, were due wholly to the diminution of the retinal image owing to their greater distance from the

eyes compared with *A* and *B*, then the same diminution ought to take place when they are at the points *H* and *I* respectively, because these points are as far from the eyes as *L* and *O*, and the retinal images must be the same.

But before referring to the possible explanation of this result we should indicate also a probable reason for the constancy of the apparent magnitude of the central circle in all degrees of convergence, a fact which would appear anomalous if we suspected that the diminution of magnitude in the first place was due to the degree of convergence. But if we note that the angle of convergence *FEG* for the simultaneous combination of *C* and *D*, and *A* and *B* is greater than that for the fusion of *C* and *D* at the points *H* and *I* respectively, and then remember at the same time both that the retinal impressions of *C* and *D* are smaller in proportion to their distance from the eyes than those of *A* and *B*, and that by hypothesis associative accommodation may coincide with the lessened convergence for *H* and *I*, we can imagine that this proportional decrease of convergence and retinal magnitude might exactly compensate for the difference of distance and for the enlarging influence of ciliary expansion or relaxation, associated with diminished adjustment. That is to say, other things being equal, decrease of convergence is, by hypothesis, connected with increase of retinal magnitude due to ciliary relaxation, habit and association having made the two functions tolerably cohesive, and hence with the approach or recession of the circles the changes of convergence might be accompanied by constancy in the apparent magnitude of the circles. But, of course, the difference between the magnitude of the fused images of *C* and *D* at *L* and *O*, and that of *A* and *B* at *F* and *G*, can be accounted for by the difference of retinal magnitude, all other things being equal in the case. This is proved by drawing circles on another card at *L* and *O* just large enough to cast the same retinal images as *A* and *B*. This can be calculated quite accurately. If then we combine them at the same time as we combine *A* and *B*, we shall find the apparent magnitude of both central circles ***exactly the same***. This would seem to justify the conclusion that, retinal magnitude remaining the same, the effect varies with convergence and combination, and

it remains only to determine whether the modification of magnitude is due to convergent or to ciliary influences, or to both of them together.

That merely retinal magnitude, so far as it is determined by the distance of the circles from the eyes, has nothing to do with the effect is amply proved by placing the larger circles, or the card containing them, at *J* and *K*, and the smaller circles at *H* and *I*. If under these conditions we combine, it will be found that the magnitude of the central circles may still be different, but the reverse of what they were in the first instance. The larger circles under fusion appear smaller than the combination of the smaller. The trouble here, however, is that the combination for both sets cannot be effected at the same time, and we have to compare a memory impression in one case with a real impression in the other. But the lapse of time is too brief to attach any weight to this objection, and besides the difference of apparent magnitude is too great in the case to make illusion possible, as every one who can perform the experiment will see. Moreover there are two objective proofs of the fact which make it unnecessary to rely upon subjective impressions as evidence. In the first place, measurement, such as has already been described, shows so marked a difference of magnitude that illusion cannot be supposed without assuming that a man cannot tell the difference between a twenty-five- and a fifty-cent piece. In the second place, we have already learned that the magnitude of the central circle in any case remains constant with the approach or recession of the figures from the eyes. In this way we can make a more or less direct comparison of images at the same time, and the result confirms all that has been described. Hence retinal magnitude may be different, so far as distance is concerned, and the effect the same, or the retinal magnitude of one image smaller than another and yet its apparent visual magnitude greater than the other. We might infer from this that the whole effect of modified magnitude was due to convergence alone, because, in spite of the smaller circles at the greater distance from the eyes, the combination of the larger circles at *J* and *K* appears smaller than that of the others.

The convergence for the larger circles is greater than that for the smaller and the magnitude is correspondingly diminished.

But there is a fatal objection to this hasty inference. We have not eliminated possible ciliary influences and the modification of the lens. By hypothesis ciliary and convergent functions are so cohesive as to act in harmony, and here, noting that the convergence is less for the remoter and smaller circles, ciliary contraction would be less, so that the effect coincides with the possible variation in the associated action of the optical lens to modify the retinal magnitude independently of distance. If this be so, we should find an actual change of retinal magnitude to compensate for the supposed differences due to distance from the eyes, and construction of the circles. But it would be a change which we should have to account for by the associative cohesion of ciliary with convergent influences, and not by reflex accommodation to distance. Hence, though the experiment may show that the effect is independent of ordinary retinal magnitude, it does not seem to eliminate that factor from the problem, because associative ciliary action may influence changes of magnitude which we may think to have eliminated.

But there are facts which strongly contradict the supposition of ciliary influence in this case as before. The first of these is one that we have already mentioned; namely, the fact that no such modification of magnitude accompanies monocular perception when one eye is covered or closed and artificial convergence effected. Under the conditions described, whatever appearances of change take place, they are purely proportional, though we should be obliged to suppose associated ciliary action along with the changes of adjustment, if that action ever takes places at all when convergence is effected. But in this case and in spite of convergence and the supposed ciliary changes the differences of magnitude are not observed. But a second experiment is still more conclusive. It is represented in the circles of Fig. 5. I draw three circles *A*, *B*, and *C*, *A* and *B* being farther apart than *B* and *C*, but in the same plane, so that combination of *A* and *B* will require greater convergence than that of *B* and *C*. Now since these circles are of the same size and lie in the same plane their retinal magnitude

will be the same. But if I cross the eyes until *B* and *C* combine, I observe two effects: first, the apparent fusion of *A* and *B*; and second, a greater diminution of magnitude in the apparent fusion of *A* and *B* than that of *B* and *C*. If I combine by negative convergence, enlargement takes place and the fusion of *A* and *B* appears larger than the apparently simultaneous fusion of *B* and *C*. Now we cannot suppose that the differences here remarked are due to differences of retinal magnitude without supposing that two very different degrees of accommodation occur simultaneously with a given degree of convergence. This must be absurd upon any theory of neural mechanics. The same absurdity is manifest if we suppose dif-

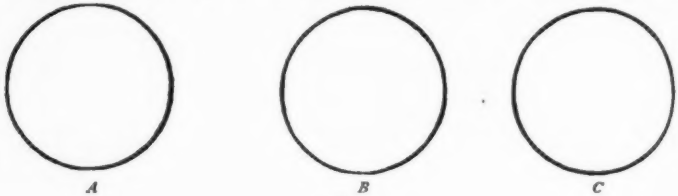


FIG. 5.

ferent degrees of simultaneous convergent tension, and with it different degrees of accommodation. The only other theory of the case that can commend itself is that of a muscle continuum which has no resemblance or analogies whatever with the ordinary theory of muscular tension, based upon the conception of mechanics. Such a view, however, only retains the motor theory in name and not in reality, and we can only wonder at the self-deception and disingenuousness that led to its adoption and the retention of all implications belonging to the muscular sensation-theory. Not that we mean to present any *a priori* objection to the general conception of such a theory, but only that it should be called a doctrine of a muscle continuum while imagining that motor implications were involved and sensory denied. However this may be, it is clear that ciliary influence and its presumably consequent modification of retinal magnitude are not available to account for the alteration of apparent magnitude in the objects in the case mentioned without supposing different degrees of accommodation simultaneously, and that, I judge, no one is willing to do. But if we

thus exclude ciliary action from the effect in Fig. 5 we have no reason to suppose that it will explain the same phenomenon in Fig. 4, and we find that the only definite generalization to be formed in the case is that apparent magnitude is somehow connected with the degree of convergence necessary to produce fusion, whether we choose to regard the influence as motor or sensory. A more complete generalization may be forthcoming in the sequel of the present discussion, after we have examined some of our experiments in the perception of distance.

There is a very pretty, and perhaps conclusive, experiment showing a modification of apparent magnitude when we *know* retinal magnitude has not been altered, whatever connection we assign between accommodation and convergence. It is in after-images, and one effect in it is quite uniform. For instance, I have often tried the experiment with pictures hanging on the wall. The attempt was to cast the after-image upon some other part of the wall nearer or remoter than the picture itself. What I have uniformly noticed is a diminution of magnitude or increase of it according as I look at the nearer or remoter point, after gazing at the picture for a moment. That is, if I throw the after-image on the ceiling, which may be farther off than the picture, as it was in the experiment here described, it appeared larger than the object; if I throw it on the wall near me, fixing my eyes upon the wall, the figure seems smaller than the original. It is to be noted also, that where I can hold the after-image for some time, which happens whenever certain neural conditions favor extreme sensibility, the image seems located at the point of fixation. When the after-image lasts but for a second this translocation is not so evident or clear, as the change of adjustment seems to suppress it. But by quick changes of positive or negative convergence I can even then notice a corresponding difference of locality and magnitude. However, when the image persists for fifteen or twenty seconds, or sometimes for nearly a minute, both the localization and modified visual magnitude are perfectly distinct. Now here is a case where we cannot plead the alteration of retinal magnitude, due to the coexistence of a certain fixed degree of accommodation with a given degree of convergence. Even if

this occurs in such cases it cannot alter the retinal magnitude of the impression, and yet we find the apparent magnitude varying with the degree of convergence while retinal magnitude remains absolutely constant. It will be apparent, therefore, that if we thus exclude the influence of ciliary contraction and expansion from the effect, and with it all fixed connection between retinal and apparent magnitude, in this important case, there will be no reason to suppose them in other cases where it is not so easy to eliminate them, and we are left only with the connection between convergence and apparent magnitude, as the only uniformity as yet unanalyzed.

There is one other interesting fact in connection with this experiment which bears upon the perception of solidity, and which should be noted before passing to that question. It is, that in spite of the influence of binocular changes of adjustment to translocate the after-image to the point of fixation, it does not alter its dimensional relations to the plane of vision. Thus, if the picture hang obliquely on the wall, say thirty degrees, more or less, and I look at it while lying on a bed or lounge and then look at the wall vertically near me, besides the diminished magnitude and translocation I notice that the image *does not lie in the plane of the wall, but in the same position relatively to the plane of vision as in its real position.* The top of the picture, which is some thirty degrees from the wall, now seems resting on the wall and the bottom of it seems much nearer. The angle at which it, the after-image, intersects the line of vision remains the same as before, and in the same relative position. At a certain point on the ceiling the image will seem to lie in its plane. This is when the plane of the ceiling and of vision concide. In all other positions the plane of the after-image and the ceiling intersect. I get the same effects from whatever position or attitude I look at the picture, and the same is true of any object from which I can succeed in getting persistent after-images. Here is a case where solidity, or the third dimension, seems independent of motor functions, even though we explain translocation and modified magnitude by them. They may modify localization without modifying the dimensional relations of the object.

But before taking up the subject of distance it will be inter-

esting to use the experiments already described to explain a phenomenon which has generally been attributed to memory and association. We may also find in it a confirmation of the suspicion that ciliary influence or accommodation may not be involved in the modification of retinal and therefore of apparent magnitude, at least to the extent required by the illustrations described. I allude to the phenomenon of the judgment of size at great distances. Physiologists and psychologists were once puzzled by the fact that an object, say a man, a horse, a wagon, or a house always seems as large at the distance of a thousand feet or more as at a distance of five hundred feet, though it was assumed that the retinal magnitude is quite different in each case. Supposing that real magnitude depends on retinal magnitude, this apparent constancy of our impression about size without regard to distance seemed to be quite an anomaly, and hence association with past experience was the resort to explain the phenomenon. But whatever influence may be attributed to association, it certainly cannot explain the constancy of apparent magnitude with the varying degrees of convergence in the experiments above described, while this latter fact throws much light on the normal impressions of mankind as containing natural and functional elements not admitted by the associational theory. It represents exactly the strong feeling we have in ordinary experience that the phenomenon is less analyzable those we find connected with association. But not to urge the force of a feeling merely, this actual constancy of apparent magnitude with the alterations of retinal magnitude and convergence, unless we assume that changes of accommodation overcome or compensate for the former, is evidence of functional arrangements for the judgment of real magnitude. But a fact in connection with the whole phenomenon of such judgments reflects upon the hypothesis of ciliary influence as affecting a modification of retinal magnitude sufficient to compensate for the effect of actual distance in diminishing it or increasing it, as the case may be. It is that beyond the parallel position of the eyes all changes of accommodation, if any, must be so independent of convergence as to cast a doubt upon the fixity of their relation within those limits. If that connection is not a firm and inva-

riable one, there ought to be some variation in the perception of magnitude with any given degree of convergence, which I do not discover. In my own case, however, it is certain that this connection between accommodation and convergence is not a fixed one, while the relation between convergence and apparent magnitude is fixed. Thus for any degree of positive convergence the perception of outline is as clear as in the normal position of the eyes. *But this is true only for the fusion of similar images.* Before the combination of the images they are slightly blurred or indistinct, and if they are different in nature, say a black and a red pencil, they are blurred even in fusion. Again, in negative convergence, as remarked earlier in the discussion, the blurring is constant until the plane on which the figures are placed is located about eighteen or twenty inches from the eyes. For this and all greater distances the images are clear. I notice in all this blurring, which is undoubtedly caused by defective accommodation, that the apparent magnitude is not affected in any way to reflect ciliary influences, and as the accommodation does not assume a definite degree until fusion of similar impressions occurs, while localization and magnitude are proportioned to convergence, there is every reason to discount the associative agency of ciliary influences and to suppose other structural functions, perhaps sensory, to account for the phenomena described, unless motor influences in connection with convergence can be invoked. Further confirmation of the unimportance, if not the absence, of association, and of the independence of convergence and accommodation, is found in the fact alluded to that blurring occurs if I close one eye and then converge while looking at an object, which shows that there is no absolute fixity of relation between the two functions as long as distinctness occurs only when fusion is effected. But if accommodation varied uniformly with convergence there should be equal clearness for all degrees of it, which is not the case except when fusion takes place. Hence I must conclude against the probability that ciliary influence determines either magnitude or localization, while the facts point to other structural functions than the purely motor to account for the effect and to explain the normal judgment of magnitude when the retinal image varies

with distance. That is to say, some other function than association and accommodation is necessary to account for the apparent constancy of magnitude in spite of distance and diminished size in the retinal image.

I come next to some experiments on the perception of distance or solidity, or, perhaps better, localization in the third dimension. They will confirm the conclusion just mentioned, and throw some light on the general function of space perception. The first illustration will involve, presumptively at least, nothing but binocular processes. If I draw four circles as represented in Fig. 6, with the centres of the

FIG. 6.

two smaller circles farther from the median plane than those of the larger, and then cross the eyes until fusion occurs, I see the usual frustum of a cylindrical cone, with the smaller base nearer the eyes. If I focus the eyes beyond the paper, negative convergence, I get the same effect, except that the relative position of the basis is reversed. The larger end now appears nearer the eyes, and the smaller end farther away. The same alteration of magnitude, in both circles, occurs as it has already been described. But these are not the phenomena to which I call special attention. The fact of apparent solidity is perfectly familiar to all observers. But I have referred to it as preliminary to the mention of certain other incidents not so generally considered, some of which I have not seen in the accounts of any other experimenter. I shall not dwell upon the relation of the circles to the median plane as a determinant of this localization in the frustum, but only upon the facts that the frustum itself seems located at the point or points of real or apparent fusion, and that the length of the frustum varies

with the distance of the paper from the eyes, and with the construction of the figures.

In the first place, if I employ positive convergence the localization is at the point of fusion or fixation between the paper and the eyes; if negative convergence it is beyond the paper at the point of fixation. These facts I determine by placing a pencil or some pointed instrument where the figure appears to be. I do not use the point to effect convergence, but after this is done I can point instantly, as in normal vision, to the places where the two bases of the frustum appear, and these are invariably as described. To do this accurately for negative convergence the circles should be drawn upon a piece of glass, and when so done I have no difficulty in indicating the true apparent position. The same is true of all objects or figures combined by binocular adjustment. A screen, for instance, always appears distinctly to be at the point of fixation nearer or beyond the real place according as the convergence is positive or negative, and my eyes can wander all over it without disturbing the illusion any more than most persons can modify the position of objects at will in normal vision. This is so universally true that I could formulate as a law that all fused images are located in the horopter, no matter what their real position in space or their distance from the eyes. At least this is true in my own experience, though subject to qualification from the disturbance of monocular functions, which varies with the nature of the figures or objects combined, being less where the resemblance or identity is great, and greater where differences of surface are marked or distinct.

I have been able to obtain an approximate measure for the length of the frustum of a cone which I see. For negative convergence this is impossible unless the circles are drawn upon glass, and even then there is the difficulty of attaining complete accuracy, because putting the pointer at the right place is not easy for the reason that it is too far away to be sure either of its exact coincidence with the visible line of the figure, or of the absence of double images. But it is quite accurate enough for practical purposes. Then there is also a limit to which even the attempt to measure the frustrum can

be made. But nevertheless the measurement can be tolerably accurate for a short distance in negative convergence, and quite perfectly so for the whole of positive convergence. This is effected in the following manner. I take a sharpened instrument, like a pencil, and place it at the point where I see one base of the frustum. This is marked by another person, and I then indicate in the same way the position of the other base. The distance between the two points is then measured. It can be quite accurate, but I have not attempted to get the most accurate results possible, but only such as give something better than my subjective judgment, and the figures will show very suggestive differences under the varying conditions. I hope that those who are devoting themselves to accurate measurements in such cases may take the matter up and investigate it more thoroughly than I have either the time or the instruments to accomplish.

Taking the circles of Fig. 6, which are drawn respectively from fifty- and ten cent silver pieces, and combining them, at a distance one foot the length of the frustum measures ½ an inch, at two feet it is ¾ of an inch, at three feet 1 inch, and at four feet 1½ inches. Beyond that distance the measurement was not attempted because I did not have at hand a pointer long enough to indicate the position of the figure. But experiment shows that the length of the frustum increases with the distance of the circles from the eyes, and the above figures will give some conception of what the general description means. There is only one anomaly to mention, and it is that my subjective judgment of the length of the frustum is uniformly greater than the actual measurement makes it. For instance, at the distance of one foot I should guess the length to be at least an inch, while the measurement puts it at ½ an inch. But the liability to this illusion is very easily explained. In normal vision the judgment of definite distance in the third dimension is never so accurate or reliable as that of plane dimension. I find by actual trial that the error is greater for median than for horizontal distances. Still it is a noteworthy fact that the measured length is much less than the apparent length.

The results of negative convergence were more difficult to

obtain and are less accurate, though they show approximate figures. The main difficulty came from the rough means employed to measure the length of the frustum, and were the differences between the lengths for various distances very slight I could place no reliance upon the result. But these are great enough to justify consideration. Another difficulty came from the blurring when the distance from the eyes was less than nine inches. Beyond this the blurring was so slight that the only obstacle to comparative accuracy was the apparent distance of the frustum from the eyes, which was about four feet. However, at a distance of three inches from the eyes the frustum, which was localized at a much greater distance, measured 4 inches; at six inches it measured 7 inches; at nine inches it measured 12 inches; and at twelve inches it measured 18 inches in length. Beyond this distance no measurements were made because of the difficulties attending it.

The length of the frustum varies with the position of the inner circle in the larger circles, being longer as its centre is farther from the centre of the larger. The reason for this is apparent as being proportioned to the amount of convergence to produce fusion. But it is not so easy to account for the increasing length of the frustum in proportion to the distance from the eyes of the plane on which the circles are drawn. If we could say that it was the association of a given degree of convergence with a given retinal magnitude we might rest content; but so far is this from being the case that a variation in the relative position of the centres of the circles always affects the apparent length of the frustum. More especially the difference between the results of negative and those of positive convergence, which is greater than the difference between the separate acts of convergence, at once eliminates the notion that association will account for the effect. For instance, the difference between the point of fixation in negative convergence at three inches distance from the eyes of the figures is about the same as that of positive convergence at three feet distance, and yet the length of the frustum in the former case was 4 inches and in the latter 1 inch, although to get any results at all by negative convergence I had to draw the circles on the glass much nearer the median line than they

are on the paper as represented in Fig. 4. Association, therefore, however much it may contribute to the effect,—and this, I suspect, is not much, if anything,—will not explain the phenomenon. But when it comes to offering anything better than association I fear that I cannot indicate anything more than the peculiar visual machinery for judging of magnitude and distance according to a very complex set of relations—the relation between the retinal image fixated and the degree of convergence, on the one hand, and between the same image and all other objects, homonymous and heteronymous, on the other. We merely know as a fact in normal vision that two circles of different sizes and at different distances, though producing exactly the same sized retinal image, the larger being the more remote, will be correctly judged as to real magnitude and distance, often under circumstances where experience and association can hardly account for the whole of the effect. Horopteral relations and their accompanying binocular functions, whatever they are, probably contribute largely to the result. That purely binocular influences may do something will be proved when we come to remark the results of rivalry between them and monocular agencies. But this name is so vague a conception that it can count for nothing except to exclude the efficiency of association to explain the result. Hence I shall not pretend as yet to have reached an adequate explanation of the phenomenon described, but must be content with stating the facts and remarking the difficulties in the way of accepting the associational theory.

There is a very pretty confirmation of this result and conclusion by a variation of the experiment. It might be said that the lengthening and shortening of the figure may be affected less by the changes of convergence than by the actual alteration of the distance of the circles. But in reply it can be shown that the same result can be obtained without altering the distance of the figures, and while they remain in the same plane. If we draw the two sets of circles on two pieces of paper, each near an edge, so that they can be moved toward or from each other, increasing or decreasing their distance from the median line at will, we shall find that the length of the frustum changes with the degree of convergence neces-

sary to produce fusion. Thus as we move the circles farther apart while increasing the convergence to retain fusion, the frustum shortens while its magnitude diminishes. On the other hand, as they approach each other and fusion is sustained the frustum lengthens and the magnitude increases, and all this while the figures occupy the same plane. The only difference is that when the circles retain the same distances from each other but are moved along the median line, that is, to and from the eyes, the magnitude remains the same while the length of the frustum changes. In this case, however, as described both undergo a modification, the contrast between the two different effects showing that one modification does not determine the other. This makes it all the more remarkable, therefore, that when lying in the same plane the third dimension should vary with the degree of convergence except that the modification seems to coincide exactly with the law established in the first case; namely, that localization in the third dimension represents varying relations of distance with the degree of adjustment necessary to produce fusion. It is hard to see that the slight changes of binocular parallax can account for the result, especially as that would seem to diminish with diminished convergence, while the length of the frustum increases under these conditions. But when the circles continue in the same plane binocular parallax increases with their increased distance from each other while the length of the frustum diminishes, and when the parallax decreases with their approach to each other the frustum lengthens. Besides there are two distinct reasons for wholly excluding parallax from the effect. The first is that the figures are in a plane, and the second is that the fused images are exactly alike, the parallax occurring between the monocular and the binocular circles, and the former taking no part in the combination. We seem left, therefore, wholly to the degree of convergence for explaining the phenomenon, retinal conditions apparently being excluded from consideration unless we can take into account something like Hering's theory of space perception. But with this view I am not yet satisfied, and if other experiments seem to exclude the muscular tension

of convergence from the effect we can only fall back upon functions which are as yet imperfectly analyzed.

There are several more interesting experiments bearing upon the question whether the perception of distance is a motor phenomenon. If I draw two circles intersecting each other, as in Fig. 7, and combine them either by positive or by negative, I obtain the following result, with the variations of magnitude already described. I see three intersecting circles, with the central one located at the point of fixation, but the two monocular circles do not appear in the same plane. They can be made to alternate with changes of attention, but without change of convergence between remoter and nearer positions than the central circle. Now they can be located beyond, and again within, the point of fixation. But the translocation in this case is monocular instead of binocular; that is, the images translocated are monocular, while the experiment described some years ago in *Mind* (vol. XIII., p. 512) represented the translocation of a binocular image with a variation of attention. In the present case, however, the binocular localization remains fixed, and the monocular images are translocated without change of convergence, though the effect is not due to the shifting of attention from the binocular to the monocular circles, but to a process like the inversion of mathematical perspective in geometrical figures. I can alternate their positions by the effort to think them within or

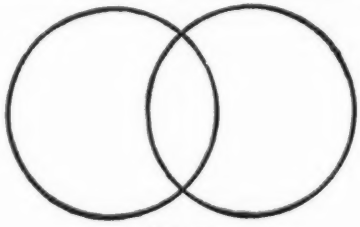


FIG. 7.

beyond the horopter. But this effort has most decided limitations. I cannot locate them thus where I please. They can occupy but either of two positions. The one beyond the horopter is considerably farther from the central circle than the one within it. This coincides singularly enough with the

tendency of divergent movements or negative convergence to increase the space representation with the degree of its condition, a fact which would hardly occur if I could determine arbitrarily the distance from the central circle at which the monocular circles should appear. But as this is a fixed position, determined by the relation of the monocular circles to corresponding points, and the function of localization, acting without any apparent change of convergence or separation of images, and as the translocation represents points which do not correspond in motor or muscular terms to the amount of tension or movement which is necessary to account for the distance between them in the median plane, and which must be the slightest in this case, if it exists at all, there seems no reason to suppose that muscular or motor efforts cause the result. There are some features about the phenomenon which look like motor influences, and which I cannot discuss at present. In a more exhaustive description I should mention the particulars, and would feel bound even to indicate and analyze them here but for several facts which depreciate their importance. *Firstly*, there is no proper proportion whatever between their localization and the utmost possible amount of muscular or motor change which might escape visual detection. A fine point in the centre of the two circles remains fused during the translocation, which represents degrees of convergence that would widely separate them. *Secondly*, I am able to translocate one of the circles to a point within, while the other remains beyond, the horopter, though this effect is not easily retained for a long period. *Thirdly*, I have been able with two sets of such circles, one immediately below the other, to get opposite effects at the same time, though only for a second or two, the result being very difficult in this case. But these facts, with the alternation of position of the two circles without the slightest apparent alteration of convergence, at least propose difficulties for the motor-tension theory unless it is made a sensorial continuum, which, as we have seen, is a retention of the muscular theory only in name. However, as already remarked, there are features about this experiment which seem to confirm the theory. I shall not emphasize its negative import, but call attention to the manner in which it refutes the

supposition of a fixed and definite connection between accommodation and convergence with a corresponding effect upon retinal and apparent magnitude. In the first place, apparent magnitude is modified with the translocation, while convergence remains unaltered. If this were due to modified accommodation, it is not a fixed degree with any given degree of convergence, and the force of supposing it such, in experiments above described, to account for alterations of magnitude, is wholly lost, and the whole result falls to the responsibilities of convergence if that be accepted as a cause in the case. In the second place, the translocation occurs without any modification of retinal magnitude, though apparent magnitude corresponds to the apparent locus of the circles. The only conclusion from this is that accommodation cannot be resorted to for an explanation of modified magnitudes, while there are also difficulties in supposing that convergence can produce the effect.

But if there are objections to the argument from the experiment just described, there is one more which makes it quite difficult to suppose that localization in the third dimension is due to muscular tension, and I shall describe it briefly. On the walls of one room in my apartment is a wall-paper of yellowish hue and blue squares formed by lines crossing it. If I cross my eyes so as to combine symmetrical portions of the surface, the diminution of magnitude and translocation of the whole surface of the wall is distinctly apparent. But it is interesting to note two anomalies. First, while all the lines or squares which are combined seem to be located in the horopter, if there be a mark or spot in any square that is not also found in the corresponding square combined with it and symmetrically located so as to fall on corresponding points simultaneously with the line of the squares, instead of being seen in the plane of the squares, that is, in the horopter, it is seen beyond it. That is to say, monocularly it seems to be localized where it really is, while binocular influences seem to affect only *similar figures and lines that fall upon corresponding points or involve the attempt at fusion.* Secondly, if a particle of dust be floating between the wall and the eyes (no reflections on my housekeeping) but beyond the horopter, represented

in the convergence mentioned, or the plane in which the squares appear to lie, as long as I watch the squares by keeping the attention closely fixed upon them, the particle of floating dust seems to be floating beyond them ; that is, beyond the horopter where it really is. But the moment that I turn the attention to the dust, without in the least altering the adjustment, and although the squares do not change their apparent magnitude nor their locality, the dust seems to be floating nearer the eyes than the squares; that is, within the horopter. This experiment is the same in character as the one referred to above as having been described in *Mind*. I have confirmed it also in another way. If I take two circles, as in Fig. 1, and draw a short vertical line at any point in one of them, preferably in the line of the vertical diameter, and then combine the circles by convergence, this short line does not seem to be located in the horopter, or plane of the central circle. It appears beyond it, although by the process described in Fig. 7 it can be translocated to a point within the horopter. But the one fact to be noted is that muscular tension seems not to affect the locus of any figure which does not at the same time represent sensory fusion.

A very interesting experiment illustrating what has just been described can be performed with an ordinary stereoscopic picture and the stereoscope. Place a mark, scar, or indenture on *one* of the pictures at any point in it, and then combine by means of the stereoscope. While the real perspective is thus developed as usual, the spot put on the picture does not seem to be localized with the points of the picture on which it is placed. It seems much nearer and so to lie in an entirely distinct plane. I have had others perform the experiment and uniformly with the same result as myself. If muscular tension had anything to do with the matter we should naturally suppose that this spot would be equally affected with all others and more especially with those in immediate contiguity with it. But such is not the case, and localization seems wholly to correspond to the fusion of identical points in the impression rather than to muscular tension. In normal conditions these two factors generally, if not always, coincide, and there is no opportunity to discover exceptions. But in the

experiment just described the muscular tension cannot be said to coincide both with the localization of the picture and the mark in it which seems nearer, and we are left rather to suppose that localization is rather connected with the tendency or actuality of fusion apart from muscular conditions.

But I shall not go so far as to exclude them from the problem altogether. It is too complicated, and so many facts consist with it that it is best to allow them a contributory effect in some cases, a determining effect in others, while appearing to be wholly absent in still others. But what functions we must appeal to to supplement the defects of the motor theory I am not yet prepared to say definitely. My experiments, so far as their effects justify any generalizations at all, may be summarized in two tentative suppositions, looking to a central explanation of both distance and magnitude, independent both of peripheral conditions and motor impulses. First, apparent magnitude is not determined by retinal magnitude, but by a certain relation between this and the point of fixation or degree of convergence. Second, localization seems to depend much more on fusion than upon motor tension of the eyes. Both these suppositions appear to make the functions of space perception central, sensory or not as you please, but not proportional to peripheral conditions, nor dependent upon what is ordinarily understood by muscular sensations. Whether such an hypothesis can be sustained or not I am not prepared to assert dogmatically, as the question is still an open one with me. But my experience makes it preferable for the present.

AN EXPERIMENTAL STUDY OF MEMORY.

BY E. A. KIRKPATRICK,

Winona, Minn.

A complete act of memory requires that impressions shall be retained, recalled, and recognized as familiar and as belonging with certain other impressions. The perfectness of any act of memory depends upon the kind and intensity of the impressions and of the associations between them. Impressions are of six kinds, visual, auditory, motor, tactual, gustatory, olfactory, but the three first named are of most importance in memory. The following experiments were made to determine which of these three kinds of impressions are best retained, and to discover the relation existing between recall and recognition.

Method.—Thirty names of common objects were chosen and arranged in three columns as follows, care being taken not to put together words commonly associated:

I.	II.	III.
box	door	pen
desk	stool	spoon
thumb	slate	pencil
chain	rug	knife
cap	hinge	shears
broom	corn	spool
sock	peach	bottle
bird	shoe	thimble
axe	hat	spectacles
post	watch	book

The test was made upon the pupils of a typical school and college in all grades from the third primary up. The words in Column I. were pronounced to them at the rate of about one every two seconds; those of Column II, having been previously written upon the board, were uncovered one at a time, and

rubbed out at about the same rate; while the objects named in Column III were shown at the same rate. In each case after the ten words were given the pupils wrote as many of them as they could remember. Three days later (in a few cases two days), at the same hour, the pupils were asked to write as many words of each column as they could remember. They were then given orally the following lists of words:

IV.	V.	VI.
loud	black	rat
bang	sparkle	spade
whisper	yellow	sheep
boom	red	rake
splash	gloom	nest
hiss	bright	mouse
buzz	green	leaf
whiz	white	hen
tinkle	shadow	cat
ring	pink	coat

They were asked to think of the sound of the first, of the visual appearance suggested by the second list, and of the objects named in the third. How closely they followed these directions it is impossible to say, but their faces indicated that they were trying to do so. There were a few indications in the papers of attempts to guess the words in Columns IV and V that they could not recall, but this was true of only a few.

Results.—The following table shows the average number of words in each list of ten recalled.

	Primary.		Gram. Sch.		High School		College.		Average.		Av. of Both Sexes.
Sex	F.	M.	F.	M.	F.	M.	F.	M.	F.	M.	
Number	15	15	39	47	58	53	50	102	162	217	379
Column I	5.46	4.33	6.48	6.17	7.29	6.94	7.20	7.38	6.90	6.79	6.85
" II	5.66	3.86	6.57	5.72	7.09	7.26	7.28	7.81	6.88	6.95	6.92
" III	7.26	6.80	8.28	7.85	8.38	8.83	8.56	8.60	8.18	8.36	8.28
" IV	5.64	5.43	6.25	6.17	7.57	6.56	7.65	7.59	7.25	6.79	6.98
" V	6.64	5.00	7.97	7.32	8.26	7.49	8.45	8.12	8.37	7.58	7.91
" VI	5.71	5.56	7.25	7.22	7.57	7.35	7.90	7.85	7.59	7.40	7.48
Average	6.06	5.16	7.13	6.74	7.69	7.40	7.86	7.89	7.53	7.31	7.40

Reproduced after 3 days.

I.	No. of words recalled.....	....	....	1.51	1.20	1.23	.97	1.79	.95	1.47	1.10	1.25
	Placed in the right column..	....	....	1.20	.79	1.00	.76	1.29	.70	1.14	.74	.91
II.	Recalled.	....	....	2.89	2.05	2.51	1.53	3.22	1.95	2.75	1.86	2.23
	Correctly placed	....	....	2.58	1.71	2.35	1.36	2.24	1.55	2.38	1.53	1.89
III.	Recalled..... ..	....	....	6.46	6.33	6.67	6.29	5.44	6.58	6.23	6.44	6.35
	Correctly placed	...	....	6.23	6.33	6.66	6.27	5.44	6.47	6.16	6.39	6.29
Recalled.....		....	...	3.62	3.19	3.47	3.06	3.48	3.16	3.45	3.13	3.27
Average correctly placed		....	....	3.33	2.94	3.30	2.79	3.32	2.57	3.22	2.88	3.03

The averages for the first three lists of words, 6.85, 6.92, and 8.28 respectively, show that objects were remembered better than the written names, and the latter better than the spoken names. Doubtless some when they saw the written words thought of the sounds and perhaps retained them as auditory words, while others thought of the visual appearance of the spoken words and retained them as visual words. Many repeated softly the words both oral and written, thus getting motor sensations; and probably many formed mental pictures of the objects named. But, however differently they may have stored the words in memory, the difference in reproducing them, since they are all of the same character, must have been due mainly to the different ways in which they were impressed. The difference would probably have been greater had the conditions for seeing the written words and the objects shown been as good for all the pupils as they were for the spoken words.

The averages for the last three lists of words, 6.98, 7.91, and 7.48 respectively, show that the visual qualities are remembered better than sounds and also better than objects imaged. In this case the impressions were all auditory, but in so far as the pupils followed directions the method of storing was different in each case. The attempt to store as directed doubtless helped reproduction in some cases and hindered in others, but in general the pupils were helped by storing in visual images, as will be seen by comparing the reproduction of the names stored in their own way with those stored as a visual image.

The results of the reproduction after three days were rather surprising. Probably the most enthusiastic advocate of object teaching would hardly have dared assert that if the names of ten common objects were pronounced to and written by pupils they would after three days remember but one seventh as many of them as they would if they were allowed to look at each of the objects a fraction over a second and write the names, yet the numbers .91 and 6.29 indicate that such would be the result. These figures indicate also that the authors of memory systems in which the kinds of associations formed are the only things considered are at fault. Some ground is found for saying, "Make the impressions vivid and the associations will take care of themselves." It is worthy of note also that there were few mistakes in recalling and recognizing the names of the objects seen. If two papers in which all the names were in the wrong column, probably because the pupils made a mistake in the column, had been omitted in averaging, the number recalled and the number in the right column would have been practically the same. This and the fact that mental images of objects are remembered better than their names is of great pedagogical significance, indicating that if objects are shown children, or when that is impracticable, if they are led to form mental images of them, they can obtain a genuine knowledge of things more readily than they can be crammed with the verbal appearance of knowledge.

The table shows an increase, from the primary department up, in the power of immediate reproduction of words of all kinds, but the difference of only about two words between primary pupils and college students is not very great when we remember that it is not so much a matter of memory as it is of mental grasp, and that the younger pupils required longer time to write the words and hence would be more likely to let some of the words drop out of consciousness. In the reproduction after three days the college students show no superiority over the children. One reason for this may be because the experiment was more of an event to the younger pupils so that the words were more deeply impressed. (In one room they discussed it afterwards, each telling how many

words he remembered.) I noticed that the third day when I told them what was wanted, the grammar-school pupils began to write at once, while the college students hesitated, apparently making an effort to recall the words. This suggests, and many other facts make it probable, that children are equal or superior to educated adults in impressibility, in retention, and in spontaneous recollection, while the latter have gained more power of *voluntary* acquisition and recollection; hence culture of the memory is not so much an increase in the power to remember as in the power to determine *what* shall be remembered. It is interesting to note that in memory of spoken words, as compared with written, the younger pupils are superior, evidently because they have not had so much practice in dealing with visual as with spoken words.

This experiment, like others that have been made, indicates that females are superior to males in both immediate and delayed reproduction of words. This difference is most marked in the spoken words of sounds, while in memory of the objects seen the boys are slightly superior.

Individual differences were very marked both as to general memory-power and memory for the different kinds of words. Only two—a ninth-grade boy and a senior girl—reproduced all of the thirty words given at one time. Nearly all remembered objects better than words, and a few who remembered words poorly were as good as or better than the average in recalling the names of the objects. Some remembered written words very well and spoken words very poorly, and others the reverse; but quite a number reproduced them equally well, and sometimes the names of objects seen also. It seemed as if they could grasp just so many words and no more, whatever, the mode of presentation. Very few gave the words in order, and it was quite noticeable that the first and last words were less frequently omitted than any others.

Some months later, in order to supplement the above experiments, the following test was made upon 180 normal-school students, the first three lists of words being used. They wrote as many of the first list as they remembered, but for the second list they were requested to simply make a mark for each word they recalled. When the third list was

given they were requested to form a mental picture of the objects named. The averages were as follows:

I. 7.33, II. 7.80, III. 8.01.

To make sure that III was not easier to remember than I the experiment was reversed with thirty-four eighth-grade pupils. The averages were:

III. 7.56, I. 8.56.

Which showed beyond doubt that the names of objects are remembered better where mental images of the objects are formed.

Three days later the normal students were asked to write all of the words they could recall, putting them so far as possible in the right columns. The average numbers recalled were:

I. 2.61, II. 1.61, III. 4.22.

And the numbers correctly placed (recognized) were

I. 1.76, II. .53, III. 3.52.

The act of writing evidently had a very marked effect especially upon recognition, since but little more than one twentieth of the words not written were recalled and recognized. How much of this effect was due to motor sensations and how much to the more prolonged attention necessarily given to the words written it is impossible to say. Movements of the vocal organs were probably also an important factor in the retention of all the words, for inquiry revealed the fact that 77 students repeated all of the words after me; and of those who did not, 32 repeated I, 26 II, and 18 III.

In order to determine to what extent the power to recognize completely or partially might remain when the limit of the power to recall has been reached, the thirty words originally given mixed haphazard with fifteen other names of common objects were read and the pupils told to write those that they thought were in the original lists, putting them in the right column as far as possible. The average numbers written were:

I. 7.43, II. 6.05, III. 8.49.

And the numbers placed in the right columns:

I. 3.75, II. 2.74, III. 6.02.

Adding it would seem that on an average 21.97 of the thirty were at least partially recognized. Since, however, on an average three of the fifteen new words were falsely recognized as being of the thirty, it would seem that six of the words apparently recognized were really guessed, leaving 15.97 as the number actually recognized. Probably the proportion was for the majority greater than this, for 47 students put down none of the new words and 30 but one. The high average of words falsely recognized was produced by a few students who depended entirely upon guessing, 18 of them putting in ten or more of the fifteen new words. The power to recognize appears therefore to be for the average student nearly double that of recall.

In individuals the difference is often much greater. One young lady that I had often noticed as able to recognize the correctness of discussions of topics that she herself could not recall was, in this test, able to recall but two words in the second list and none in the first. When the words were read, however, she correctly recognized seven of the first list and five of the second and wrote none of the fifteen new words. There were other students who recalled a great many words, but placed but very few of them in the right lists.

There were some incidental illustrations of false recognition. About a week previously in experimenting upon mental imagery I had pronounced to the normal students ten common words. Many of these were recalled and placed with the memory list. Again, it appears that when such words as 'spool,' 'thimble,' 'knife,' were pronounced many students at once thought of 'thread,' 'needle,' 'fork,' which are so frequently associated with them. The result was that many gave those words as belonging to the list. This is an excellent illustration of how things suggested to a person by an experience may be honestly reported by him as a part of the experience.

The results of these experiments are of more special interest to those concerned with pedagogical problems. They

reveal very clearly the absurdity of the common assumption that any subject that requires memorizing gives valuable memory-training, and suggest for further observation and experiment questions which when answered will enable teachers to intelligently direct the exercise of the memory of pupils in general and to correct special defects in individuals.

DISCUSSION.

THE ORIGIN OF EMOTIONAL EXPRESSION.

Recent discussion has brought out certain great facts about the psycho-physics of emotion. The service of the 'peripheral' theory as announced by Lange and James, and especially as argued by the latter, has been to set this problem in evidence. Undoubtedly the stimulating and highly valuable influence of James's treatment—here as on many other points—has been due to a certain frankness and *naïve* clearness which has concealed in a measure the real complexity of the problem.

The outcome of a discussion in which this 'peripheral' theory has had able but, I think, very inadequate criticism takes form about two or three general principles which I am now aiming to state in their general bearing upon the origin of emotional expression. It has been evident from the first that the 'emotion' that the peripheralists are talking about is a phenomenon of instinct—something that a baby has already got; and that the emotion that the adversaries of the theory are talking about is a phenomenon of ideas—something that the baby has yet to get. If this be true—and no one denies the distinction in fact, apart from the terms which have hopelessly obscured it—it becomes just as evident that the question as to what the components of emotion are is really a genetic question. All the elements of the problem of the genesis of reactions—that of the laws of motor development—must be recognized and woven into an adequate theory. This is what I mean by saying that the 'effect' theory of emotion is *naïve*—just as much so as the old 'cause' theory.

When, then, we come to take a broad survey of motor development, in the race no less than in the child, we are able to signalize certain great principles which we cannot do without: principles which stand out in biology and in psychology as essential to any theory of development. The range of facts fairly shown by recent discussion to be available for the genetic theory of emotion-reactions may be stated under three such principles. These are *Habit*, used broadly to include the effects of repetition and heredity, as the postulate of 'race-experi-

ence' makes use of it; *Accommodation*, the law of adaptation in all progressive evolution, no matter how adaptation is secured; and, earliest and most fundamental, *Dynamogenesis*, expressing the fact simply of regular connection between the sensory and motor sides of all living reactions, as to amount of process.

I. As for the fact of dynamogenesis: who doubts its force, either in race-history or in the life of the single organism? I have been so sure of it that I have made it the ever-present fact in the whole analysis of the 'motor consciousness.'* Fouillée writes a whole philosophy to expound it.† And as for the advocates of the theory of emotion now in question, no one has done more to prove this truth of dynamogenesis than Féré,‡ and no one more to illustrate it than James.§

But what bearing has this principle upon the theory of emotion? Much every way. We must bear in mind that this principle has always been acting, and always is acting, in every reaction we make; that our reactions have grown to be what they are in all cases by direct reflection of what we have received or experienced; that just as certain as it is that we are experiencing new things every instant of our lives, just so certain is it that we are expressing these new experiences in every reaction that we make. Every one is familiar with Prof. James's view that we never have the same mental content twice. Of course we do not; there was nothing really new about that. But the correlative fact has not had recognition. If we never experience the same twice, so we never *act* the same twice. The new x of content, added to the old c of content, must call out a new x of action, added to the old a of action. If then our reaction is always $a + x$, just as the content which it follows upon is $c + x$, then no reaction is ever that and that only which is guaranteed by habit, inheritance, and what not, in the past.

It is easy to see, however, that the 'effect' advocate has a way of escape from any such easy trap. He admits it all, and adds a pertinent view. He distinguishes content + its expression from content + *feeling* of its expression; saying that there is no consciousness or feeling of the new element of motor process until it is itself reported as a new element of sensory content. Quite possible; it may be so: if the nervous system has developed that way. But the question whether it has developed that way resolves itself into the more theoretical one, how could it develop that way? That is, assuming that it has, what view must we then hold as to the actual mode which the organism has of acquiring reactions to new elements of content; or,

* See my *Feeling and Will*, Part IV.
† *Psychologie des Idées-Forces.*
‡ *Recherches sur la Sensibilité.*
§ *Principles of Psychology.*

in short, of acquiring any new reactions? This clearly takes us into the domain of another of the principles of development mentioned above, Accommodation. But before we turn to that, certain deductions favorable to the theory in question may be shown by what precedes to be necessary from the third of our principles, Habit.

II. It is now evident that a motor reaction of any kind has always two stimulating antecedents: one the influence fixed by habit, and the other the influence of the new elements of content presented by the environment. But we know that habit tends to make reactions automatic and reflex; and that consciousness tends to evaporate from such reactions. As I put it long ago, "psychologically, it [Habit] means loss of oversight, diffusion of attention, subsiding consciousness." * Hence we must admit that the reactions most dominated by habit—the smoothest, most inherited, most instinctive reactions—have *least consciousness*. And, on the other hand, where habit is least influential, where the content is largely new, where the pleasure or pain of its assimilation is great, where attention and effort are strained, where excitement runs high—in all these cases the stimulating influence is new, one which has not yet been brought under the influence of habit, and so one which adds a new dynamogenic influence to the reaction.

It turns out, however, that just those 'expressive' reactions which are most instinctive and reflex (fear, anger, joy, etc.) really do carry with them most of the consciousness which we call emotion—certainly vivid and disturbed enough. What then shall we say? Either that there are other new elements of content additional to the regular antecedents of the reflex; or that the emotion is not the antecedent of the expression at all, but that the reverse is true—the emotion is consequent upon the expression. We cannot hold to the former alternative. Where are the adequate stimulants in conscious content to the newly-hatched chick's wild fear of the hawk? So we must take the other alternative, and *hand over all this class of reactions to the effect theory*, admitting that the emotion, as far as it has fixed instinctive forms of expression, follows upon the expression. I have no hesitation, therefore, in adopting the 'effect' theory of Lange and James as regards inherited emotional expression excited by constant definite objects of presentation. But this argument for it has not been made before, I think.

This is therefore no exception to our law of ontogenetic expression, i.e., the law that that which is habitual is accompanied by least consciousness. The high consciousness is a reflex effect. But we would

* *Feeling and Will*, p. 49.

expect, on the other hand, that in all the ideal states of mind, in all the new complications of content to which the attention gets adjusted, in all emotional states which do not attach immediately and unreflectively to conscious objects of presentation,—that in all these cases the exciting influence should give elements of expression over and above the reactions due to habit. This is really the outcome—and about the only valid outcome—of the numerous criticisms of James recently made from different points of view.

But it must be remembered that a claim is still open to the 'effect' theorist, as was said above; namely, that even though this be true, still the central process at the base of it may not itself get into consciousness as emotion. It may get in to consciousness only as presentation, attention, etc., the emotion-consciousness not arising until the reaction thus stimulated is *reported back* from the periphery. But, again, this whole question of the behavior of the organism in the presence of the intellectually new as opposed to the habitual is only another stage of the question spoken of above; i.e., the behavior of the organism in the presence of new sense-stimulations. How has the organism been able to acquire new reactions *of any kind*?—a genetic question and a fundamental one. This leads us again to the principle of accommodation, to which I now turn.

III. The principle of 'accommodation'—the psychological phase of the biological problem of adaptation or development—is the most urgent difficult, and neglected question of the new genetic psychology. How can consciousness ever, under any circumstances, get a new and better-adapted function? In answer to this question there is only one theory in the field, that of Bain, in his latest formulation of which he shows its conformity to evolution requirements. Spencer's theory is purely biological and seems to be open to some of the modifications (and more) suggested by Bain in the following passage,* which is his latest utterance, I think:

> "My leading postulates—Spontaneity, the Continuing of an action that gives pleasure, and the Contiguous growth of an accidental connection—are all involved in Mr. Spencer's explanation of the development of our activity. . . . The spontaneous commencement is expressed by him as a diffused discharge of muscular energy (*Psychology*, vol. i. p. 544). He considers that as nervous structures become more complicated, every special muscular excitement is accompanied by some general muscular excitement. Along with the concentrated discharge to particular muscles, the ganglionic plexuses inevitably carry off a certain diffused discharge to the muscles at large; and this diffused discharge may lead to the happy movement suitable to some emergency.
>
> "This is the doctrine of Spontaneity in a very contracted shape; too contracted in my judgment for the requirements of the case. I have adverted to the inferiority of the diffused wave accompanying a ceniral process, whether active or emotional, such as is here assumed. If another source of chance muscular movements can be assigned,

**Emotions and Will*, 3d. ed. 1888, p. 318 f.

and if that source presents advantages over the diffused discharge, we ought to include it in our hypothesis . . . Mr. Darwin expresses what is tantamount to the spontaneity of movement thus: 'When the sensorium is strongly excited, the muscles of the body are generally thrown into violent action.' 'Involuntary and purposeless contractions of the muscles of the chest and glottis, excited in the above manner, may have first given rise to the emission of vocal sounds' (*Expression*, pp. 82, 83). This is spontaneous commencement under circumstances of strong excitement; but I have endeavored to show that excitement is unnecessary, and that spontaneity is a fact of the ordinary working of the organs.

"The second indispensable requisite to voluntary acquisition, as well as to the consolidation of instinctive powers, is some force that clenches and confirms some successful chance coincidence. Mr. Spencer's view of this operation is given thus: 'After success will immediately come pleasurable sensations with an accompanying large draught of nervous energy towards the organs employed.' 'The lines of communication through which the diffused discharge happened in this case to pass have opened a new way to certain wide channels of escape; and consequently they have suddenly become lines through which a larger quantity of molecular motion is drawn, and lines which are so rendered more permeable than before.'

"Here is assumed the Law of Pleasure and Pain. Pleasure is accompanied by heightened nervous energy, which nervous energy finds its way to the lines of communication that have been opened up by the lucky coincidence. There is assumed as a consequence the third of the above postulates—the contiguous adhesion between the two states, the state of feeling and the appropriate muscular state. The physical expression given by Mr. Spencer to this result is, I have no doubt, correct—'the opening up of lines of discharge that draw off large amounts of molecular motion.'"

Bain's three postulates touch the inevitable requirements of a theory, i.e., first to get movements (his 'spontaneity' as a substitute for Spencer's 'diffused discharge' and Darwin's 'purposeless contractions;') second, to get selections made from these movements (his 'accidental success' of certain movements); and third, 'some force that clenches and confirms some successful chance coincidence' ('pleasure and pain,' identified with Spencer's 'heightened nervous energy which finds its way to the lines of communication that have been opened up by the lucky coincidence').

I do not intend to go into a criticism of this scheme in detail, especially as I intend soon to publish a book containing a detailed theory of accommodation.* But it is evident that the truth—if it be true—of 'spontaneity' in developed organisms does not invalidate or even supersede Spencer's 'diffused discharge'; for the phylogenetic explanation of spontaneity—the question how did spontaneity arise—must rest on some such hypothesis as Spencer's. But the question comes: given movements—by either of these principles, both, or neither—how are some of them selected and preserved? Here again the answer comes from both authors: the fitness of those selected, by the application of natural selection to movements overproduced in quantity. This we may admit as most likely. But now—and here we reach our

* A sketch of some of its features may be read in my article on *Imitation* in *Mind*, Jan. 1894.

topic again, emotion—how are these successful, good, advantageous movements kept up? 'Pleasure and pain' is at once on everybody's lips, Bain's, Spencer's, *et al.* But how? Evidently by association, we are told. The lucky movement gives pleasure; it is done again to secure the pleasure again. But we may say: for an association one term must be given; either the pleasure to bring up the movement, or the movement to bring up the pleasure. We must have the presence of the one in some kind of 'organic memory' in order to get the presence of the other. How does the organism get started toward either? Here Mr. Spencer's theory, on the organic side, gives us an answer; and Bain, as it seems to me, adopts it as a supplement, in the quotation made above from his third edition, directly from Spencer. "Here is assumed," says Bain, "the 'law of pleasure and pain.' Pleasure is accompanied by *heightened nervous energy*, which nervous energy finds its way to the lines of communication that have been opened up by the lucky coincidence."

Let us say, then, that something equivalent to 'heightened nervous energy' alone can explain the repetition of useful and pleasurable reactions. After sufficient criticism and definition—which I now reserve—we may call this for convenience the principle of 'Motor Excess,' and say that pleasure and pain can be agents of accommodation and development only if the one, pleasure, carry with it the phenomenon of 'motor excess,' and the other, pain, the reverse—probably some form of inhibition or of antagonistic contraction. On this basis Darwin's well-known 'laws' get their application.*

What has this to do with emotion? Again, much every way. The heightened nervous energy may not be—without further argument now out of place—assumed to be the 'excitement of emotion'; and we may be dealing only with the pleasure-pain process: but even so, our analogy is worth something. Let us ask this question: where in the entire series of events—stimulus, central process, reaction—has the pain come in, before or after the first adopted movement, i.e., the pain that has an inhibiting influence on this movement? The whole phraseology of Spencer and Bain would serve to make us think that it came in *after the movement only*, as part of the effect of the movement, so that, by the memory of the pain thus got, the movement is inhibited. The pain got from the movement serves in memory to warn us not to repeat *the movement.*† But here I take issue blankly, contending that

* Cf. Mr. Dewey's article in this number of the REVIEW, of which I have seen proof-sheets.

† In support of this see Spencer, *Prin. of Psych.*, vol. I. §§ 227 f., §§ 232, 237. Bain's view is seen in the quotation given above.

it comes in *by and in the stimulus* and *before* its discharge in movement, warning us not to move *so as to repeat that stimulus.* It is by this effect that the first adaptive movement is secured.

Let us take for scrutiny the customary illustration—the one which James uses in explaining the 'Meynert scheme' of nervous action. A child thrusts his finger in a candle-flame, and is burned: he thrusts no more, but shrinks. Here the doctrine of Spencer, Bain, and many others, seems to make the function of the pain the inhibition of the thrusting movement, as itself undesirable. But surely the case is very different. The inhibiting effect and the pain are brought about by the burn, and the recurrence of *that*—that is the thing to be prevented. The thrusting movement is a mere incident. Suppose the candle is brought up against the child instead of the reverse: it then shrinks from it just the same. The movement of the former case is inhibited, to be sure; but only because that is the way the developed organism *has learned to escape damaging stimulations* in general. But how it got this way of escaping them, that I contend is just what we are trying to explain—the fact of habitual expression. The real question is: how did the organism learn to withdraw? And the answer must be: the pain must have originally preceded the adaptive movement—as a signal of an injury. And this original differential motor effect of stimulations can only have been acquired by natural selection.

We cannot simply leave the organisms to the risks of getting repetitions of stimulus by accident; for that means that the organism waits the second time for the lucky chance, just as it did the first time: and that is to say that the pleasure of the first experience left no effect which by its mere presence *increased* the chance of luck.

So it follows that, as we had to hand over to the effect theory all the instinctive expressions, as being so reflex that there is no consciousness of them until their organic resonance is borne back to the centres, so now we see that *in its origin* each and every one of them was directly expressive of a state of consciousness, under the law of accommodation by pleasure and pain. These expressions have grown up, by such principles as Darwin's, as accretions to Habit; but only because they at first followed definite pleasure-pain processes.

This principle applies also to all the organic, visceral, conæsthetic sensations so vital to many emotions. For we are of course driven to ask how it comes that habitual reactions, by becoming more reflex and hence less conscious, come to give, nevertheless, by their return wave upon consciousness, such overpowering floods of organic feeling. I think it is due to the fact that it is by muscular movement that these violent often long-continued protective or offensive reactions are carried

out. This exhaustive muscular process taxes for its maintenance all the organic processes—heart, lungs, etc.; so that a great mass of organic sensations are thrown into consciousness, and by unbroken association get to stand themselves, in union with muscular sensations, for the damaging or beneficial *kinds of stimulation* that at first excited pleasure or pain. And as far as they were themselves exhausting and devitalizing, they were directly painful. It is common doctrine that in our more violent organic reactions in emotion, the organism is recapitulating in amount the wear and tear of the long process of offence or defence that animal forms were accustomed to go through when they met the objects which now excite these emotions and sensations in us.

My charge therefore is this: the effect theory cannot be true from the point of view of the development or phylogeny of the pleasure-pain consciousness. And the argument is this: If (1) our theory makes use of pleasure and pain as an agent of development, it must make this pleasure and pain antecedent in the beginning to the reactions which stand for the adaptations secured by the pleasure and pain. The Spencer-Bain theory makes memories of pleasure or pain antecedent to the repetition or inhibition of movements, but it recognizes no pleasure or pain *quâ* stimulus to the original adaptive movements. Otherwise we have a dualism on the account of development, i.e., pleasure-pain securing adaptations, and pleasure-pain emotions resulting from adaptations. No doubt both of these *are true as facts in developed organisms:* but we are now talking about *origins.* One of them must be original.* As I have said before succinctly: "The analogies from sensuous feeling [sense-pleasures] are all in favor of the central origin of emotion [idea-pleasures]. No one holds that sensations are felt only as far as they have motor expression. The kinæsthetic theory accordingly forfeits unity in its account of [the development of] sensibility." † If (2) the effect theory do not make use of pleasure and pain as agents of development, then it owes us a theory of development both of sensibility and motor acquirement, for it throws away the Spencer-Bain theory. Such a theory would have to rest, as far as I see, upon 'lucky' chance alone, going back to Bain's early view—before he supplemented it with Spencer's 'diffused nervous discharge'—and developing all movement, voluntary as well as reflex, by simple chance repetition with association. This further, as I have urged, makes an illicit use of the principle of association.

* See the reference to Marshall's 'dualism' below. James attempts to bring the sense-pleasures and pains under his theory, in a recent discussion (see my 'postscript' to this article below).

† *Feeling and Will*, p. 256. Cf. the detailed criticism of Worcester, in *Monist*, Jan. 1893, p. 285.

This latter is the view advocated often by biologists; even those who, as in a recent case,* appeal to psychology for analogies of development. I have before admitted the possibility of such a theoretical view, as regards some slight organic development; but I think with Spencer, Bain, Sully, etc., that it fails to account for any very complex motor acquisitions. It emasculates higher psychological theory by throwing over the teleological function of pleasure and pain—just the one thing which comes into clear consciousness in this matter of development. On such points I think psychologists ought to give some healthy instruction to the biologists.

The place of sense-pleasures and pains, therefore, in my genetic theory, throws light at once upon the theory of emotional expression. Such pleasures and pains are not only indices of organic weal or woe; they are also dynamogenic agents of accommodation, by a direct influence on muscular movement.† And the very same considerations apply also to ideal pleasures and pains, those, e.g., which cluster about phases of attention, ideation, and object-consciousness generally. Intellect could not have developed in the first place, nor have become the magnificent engine of organic accommodation, through volition, which it is, if intellectual, æsthetic, and ethical pleasures were only the resonance of instinct-reflexes. Yet even here many of the qualitative marks, the excitement, the main psychosis apart from the pleasures and pains of new apprehensions, knowledges, curiosities, are just as surely, and for the same genetic reasons, the resonance of instinct-reflexes as are the gross fixed expressions of anger, fear, etc., in animals.

The immediate locus of the hedonic element in most highly-toned psychoses is therefore sensory and central (i.e., not a matter of reaction or 'resonance'), and can only be 'kinæsthetic' and motor in two cases: first when by habit the reaction has become reflex and the emotional storm bursts into consciousness with its organic associations by a return wave from the motor and organic reactions; or when the original pleasure or pain was itself an index of a muscular condition or function.

And we may go a step further and point out that even when the

* Orr, *Theory of Heredity and Development*, pp. 95–101, who bases his theory of development upon the psychological principles 'repetition' and 'association,' and takes no note of the value of pleasure and pain, or their nervous equivalent, in the process.

† I have elsewhere insisted (*Feeling and Will*, chaps. v and xi) that the traditional 'welfare' theory of pleasure and pain must be modified to apply to 'prospective' experience—as an agent of accommodation—if it is to be available in organic development. This contention, like all other genetic considerations, has had little recognition. Dr. Nichols is, in my opinion, perhaps the only recent writer on this subject who sees the importance of origins.

pleasure-pain element is thus reflex, an element in some sort of utility-reaction established by habit, it then nevertheless still plays the original rôle also, i.e., it becomes the index of the relative advantage to the organism of that same utility-reaction in the newer conditions of life, and so tends to secure yet another secondary reaction. In this way while the pleasure-pain process may by constant association get to be part of a sensation, or a whole sensation with a nerve-apparatus of its own, it then also serves, as all other sensations do, to start its own motor expression in some such antithesis of out-and-in movements as that suggested independently and on different grounds by Münsterberg and by myself. This latter reaction is then 'toned' centrally, as the original utility-reaction was, and contributes a new hedonic element to consciousness. We thus have a certain genetic justification for the distinction between 'agreeableness' and pleasure on one hand, and 'disagreeableness' (*Unlust*) and 'pain' (*Schmerz*) on the other.*

Genetic conditions therefore—to sum up—require that there should be three elements in all emotion: (1) an habitual and in the main inherited element, due to a 'return wave' from various instinctive expressions; (2) a present 'accommodation' element of pleasure and pain produced in consciousness by new sensory, intellectual, and ideal processes; and (3) a 'return wave' element from (2) and from muscular and organic processes in vital connection or association with (1) and (2).

The peripheral or 'effect' theory accounts for the presence of (1), and for (3); it does not account for the origin of (1), nor for (2).

The necessity for some such genetic reconstruction of the doctrine of emotion—to straighten out the tangled lines of fact—may be seen by the examination of a recent book in which many of the salient facts are stated with commendable clearness, but which in my view yet fails signally to unite them.† Mr. Marshall, by dubbing emotions instinct-feelings, goes so far—as James had also—to do justice to the fact of Habit in fixing emotional expression; but then he goes on to deny the adequacy of the effect theory of these instinct-feelings. He seems to suppose that there is a mental accompaniment of marked quality attaching to every instinct apart from its return wave of expression. But a genetic view of Habit would have saved him this; for everybody admits that the greater the fixity of habit the less the consciousness, and instinct is usually quoted as the best instance of this very truth.

* This point receives fuller notice in its proper theoretical position in my forthcoming work on 'Mental Development.'

† Marshall, *Pleasure, Pain, and Æsthetics.*

But Mr. Marshall excepts from the definition of emotion, purely on genetic grounds, two great classes of reactions which nevertheless have emotion accompaniments, i.e., imitative reactions, and what I may, with his approval, call 'self-exhibiting' reactions. These are not adequately fixed in instinct combinations, because their range of content and adaptation is too great to allow them to be shut down to definite channels. True again and good, although I by no means accept this classification of such reactions. But the fact of them illustrates the great genetic principle of accommodation; and if Mr. Marshall could bring himself to take a more genetic view, he would see that all the reactions which are now instincts were once in exactly the same state, but have become consolidated in definite ways upon definite objects. It would then be clear that all emotion is, *in its origin* and process of making, largely a central phenomenon of pleasure and pain, but that all emotion *in its development* is becoming a peripheral and organic phenomenon of 'resonance' or reaction, according as, by the law of Habit, consciousness falls away from the business of the centres and attaches more and more to the business of the periphery.

So Mr. Marshall is then driven to a dualistic view of the affective life in its totality. He agrees with every one in making 'pleasure-pain' and emotion both, as it were, thermometers (or why not *algedometers?*) of development, the indications in consciousness of some sort of good or ill fortune. But he is forced to find them to be different thermometers for registering the same scale of temperatures—to carry out an inadequate figure. He himself has brought the same objection to the 'well-being' theory of 'pleasure-pain,' i.e., it should give two lines of development;* and then commits the very same genetic error as respects pleasure-pain in contrast with emotion. He says (p. 93 f.): "The two sets of phenomena are allied in that both are primitive; in both cases we are able to trace their genesis back to the earliest developments of consciousness; both guide towards the advantageous and away from the disadvantageous."

In arguing this dualism by an analysis of the developed emotional consciousness, Mr. Marshall makes out his case again, I think, and adds one or two new and important *aperçus*, such as the difference between pleasure-pain-reactions and emotion-reactions, and such as the claim that pain expression can be inhibited without inhibiting the pain, while the same is not true of emotion. But when he says that "in both cases we are able to trace their genesis back to the earliest

* A criticism which is wide of the mark, since all the evidence goes to show that pleasure and pain represent complementary organic processes. Meynert's reaching and withdrawing, etc.; the above-freezing and below-freezing on the thermometer.

developments of consciousness," it only remains to ask, why does he not do it? That is just the genetic task which I am undertaking now, and which Mr. Marshall dismisses in such words as these (p. 85): "The value of such argument, doubtful . . . even as far as we have gone, becomes more so the farther we proceed, because of the uncertainty as to the history of our racial development."

To show it would require not only some such hypothesis as Spencer's 'heightened nervous energy,' as the basis of Bain's pleasure-pain factor, but also another kind of heightened nervous energy—for what else could it be?—to represent emotion. Is it not evidently better to say that one sort of heightened nervous energy does for both, and that the conscious difference is due to the different sensory elements which come in together with the pleasure-pain? In sense-states we have pleasure-pain—*Gefühlston*—plus sense-quality (visual, auditory, etc.); in emotion-states we have pleasure-pain—*Vorstellungston*—plus sense-quality (muscular, organic, etc.). The difference, then, is one of developmental level. This seems to me to be fully covered by my hypothesis stated above that pleasure-pain represents the new accommodations, both at the beginning and at every stage of development, and that emotional expression represents the habits organized on the basis of the perception and recognition of objects. The possible dualism then is that between pleasure-pain and sensation.

Postscript.—Prof. James's remarkable clearing-up article on 'The Physical Basis of Emotion' in the last number of this REVIEW calls for an additional word of comment. This paper of his practically settles the controversy over his theory, I think. It shows that we have all misunderstood his book and also, I may say frankly, that he is to blame for the misunderstanding. In my opinion, he now states a theory so different from that in his book that it is fair to say either that criticism has driven him out of his old position or that what he has himself called 'slap-dash' treatment—I call it above (written before his paper appeared) '*naïve*' treatment—misled us all. At any rate, no one should now read, much less teach, his book without practically substituting this article for his chapter on 'Emotion.'

In his new statement, Prof. James claims three elements in emotion: (1) the *direct* reverberation or reaction element, a setback from muscles, organs, etc., in consequence and *contrast* to the incoming stimulus which brings such reactions about. This element is so excessively emphasized in his book that most of his critics have supposed he meant this alone. But to refute all such he now, perfectly legitimately, I think, brings out the second factor in emotion, i.e. (2) the *associated*

mass of content—ideas, etc.—which hangs together, however remotely, with the direct reverberation, and so secures all the power of association as an explaining agent. This he urges with great strength in this article, smiting most of his critics hip and thigh. This principle is fairly included by inference, I think, in his book—although so feebly and dimly that the blame is really his that so much good philosophical print has been spent in making the objections to him which he now fully and clearly sweeps away. I must add that I would not have supposed that he himself had thought out these associative extensions to his theory when he wrote the 'Emotion' chapter; for he must have seen that to present them would strengthen his book to an enormous degree. But granted the success of the 'association' element which Worcester and others so plainly overlook, Prof. James now brings in his third element in emotion, i.e., (3) all pleasure and pain tone in consciousness due to 'incoming currents.'

Now to say that the *Gefülston* of sensation, admitting that it is involved in the sensation process itself and is not due to a reaction or reverberation, "falls comfortably under my [his] theory"—this is to blow the frog of his original theory up big enough to rival the ox. Why! who is there to oppose a theory which covers everything so 'comfortably'? I know of no one, unless it be some radical Herbartian who holds that central *Hemmungsprocessen* can go on in the brain entirely apart from sensory conditions and 'incoming currents.' If Prof. James has meant all along what he now says, then I for one have shown in what I have written in the foregoing pages about pleasures and pains of 'accommodation,' both sensory and intellectual, that I agree with him; but it was with a very different understanding of his views that I wrote the pages above which include the passage quoted from my book (*Feeling and Will*, p. 256).

That I am now right in saying that in his original chapter he takes no account of any elements but those of resonance, muscular and organic, is shown by the following quotations. Under the caption 'Coarser Emotions' we read (vol. II. p. 458): "Each emotion is the resultant of a sum of elements, and each element is caused by a physiological process of a sort already well known. The elements are *all organic changes*, and each of them is the *reflex effect* of the exciting object" (italics mine). And under the caption 'Subtler Emotions' (II. p. 471) "In all cases of intellectual or moral rapture we find that unless there be coupled a reverberation of some kind with the mere thought of the object or cognition of its quality . . . our state of mind can hardly be called emotional at all. It is in fact a mere intellectual perception of how certain things are to be called. Such a judicial

state of mind is to be classed among awarenesses of truth; it is a *cognitive* act" (italics his).

Moreover, Prof. James now sees that he agrees with his critics except on one point, which I think it is the main merit of the whole discussion to have brought to the front. He says:* "It may be after all that the difference between the theory and the views of its critics is insignificant." Why? Because—and the following passage shows that it is not James's theory which has become 'orthodox,' as he hopes, but James himself—"The only feelings which I myself cannot more or less well localize in the body are very mild and, so to speak, platonic affairs. I allow them hypothetically to exist, however, in the form of the subtler emotions, and in the intrinsic agreeableness and disagreeableness of particular sensations, images, and thought-processes where no obvious organic excitement is aroused." It is true that he made such an admission in his book regarding 'subtler emotions'; but it seemed clearly contradicted by the context, and I was one of those associated with Lehmann and Irons who said that such an admission would 'give away' the whole theory. Nor do I think we were guilty of an *ignoratio elenchi*, as he now says, although we certainly would be to repeat the charge now.

The final point which James's article sets in the focus may be put in his words: "Must we admit, in the complex emotional state of mind, an element . . . distinct both from the intrinsic feeling-tone of the object and from that of the reactions aroused—an element of which the 'liking' and 'repugnance' mentioned above are the types, but for which other names may in other cases be found?" "Are *these* a third sort of affection, *not* due to afferent currents, and interpolated between feelings and reactions which are so due? Or are they a name for what . . . resolves itself into more delicate reactions still? I incline to the latter view."

I also incline to the latter view and hope soon, in my new book, to give some genetic reasons for so doing. So I am only too happy to say that I am now with Prof. James all along the line, and I hope he may see in the genetic positions stated above some further grounds for his views. But we may still ask—those of us who *now* agree with him, for we are probably many—who has been converted to orthodoxy?

J. MARK BALDWIN.

PRINCETON UNIVERSITY.

* This REVIEW, I. July 1894, p. 524.

PSYCHOLOGICAL LITERATURE.

Le sentiment et la pensée et leurs principaux aspects physiologiques. Essai de psychologie expérimentale et comparée. ANDRÉ GODFERNAUX. Paris, Alcan, 1894. Pp. xix + 224.

This very readable and suggestive book bears a major thesis and several minor theses. The major thesis is the well-known one of 'parallelism' or 'double-aspect,' according to which consciousness is the accompaniment of a continuous chain of 'motor phenomena,' between the links of which the causal relation is exclusively to be sought. The minor theses, as I read the author's meaning, are, first, that the motor phenomena in question are actual muscular contractions and tendencies to contraction; second, that these contractions and tendencies are primitively realized in consciousness as feelings or affective states, rather than as percepts or ideas; third, that they form emotional conditions and moods of feeling so long as they remain unsystematized and diffused, and that at such times thought proper or ideation becomes either inhibited or incoherent; fourth, that they contract adhesions with the elements of thought proper, and then, when they run in well-organized channels, cease to be felt affectively, whilst the ideas or thoughts with which they are connected occupy the surface of consciousness; and finally, fifth, that in this subconscious state the motor tendencies nevertheless form the associative links between the ideas, so that sentiment must be said to determine ideation and not ideation sentiment, and the affective life, in a word, with its deeps and shallows, appears as the shifting sea-bed over which all our thought passively floats.

This, the reader sees, is a very fine programme of psychologic theory for a young author to start upon life with, and many future years of industry may well be spent by him in working out and strengthening its parts. At present he modestly admits that its proofs are imperfect in many places; and I must confess that, as I read it, some of its essential elements seem far from being clearly conceived. In the parallelism theory itself, for instance, previous authors have usually imagined the 'motor phenomenon' to which the conscious one corresponds to be the nerve-current or discharge from one region of the cortex to

another. This kind of motor phenomenon might conceivably be, at times, a direct link between ideational centres, so that ideas might well follow upon each other as its consequence, without any specifically muscular tendency or contraction being interposed. M. Godfernaux simply passes this hypothesis over in silence, and one misses in his frequent references to his own theory any definite schematization of the way in which the ideational beads get strung upon that thread of muscular 'tendencies' which he relies on for the associative work. Another point which I find obscure is this author's conception of the 'systematization' of his motor tendencies. In the purely ideal world we have systems realized before us at every moment of our lives. Ruling purposes ministered to by successively-subordinated means, organized wholes with parts involved, general heads with subdivisions under each, whole trees of thought, in fact, with their branches and twigs complete, are among the most constant objects of our contemplation. Here is 'synthesis,' realized and understood as fast as represented, and only made difficult of comprehension by that false theory of association which insists that all ideas are 'loose and separate,' and that each knows its own elementary object, but nothing in the world besides. Once admit an 'idea' to be capable of knowing a whole system of things at once, while the particular things that make up the system are determined by the concomitant brain-processes, and there is no farther paradox. M. Godfernaux, however, conceives the ideas in the Humian way, and rightly saying (pp. 160, 161) that the English school cannot explain how, when so few of them can be at once in consciousness, our mental syntheses and systems can imply so many, thinks he sees his way out of the difficulty by invoking muscular tendencies. These tendencies, he says substantially, are themselves branched like trees. There is always a fundamental or synthetic one, diffused and vague, which decomposes itself into others more concrete and determinate, of a similar sort, the whole forming a co-ordinated system of channels of discharge, adapted more or less to the environment, and bearing on their extremities, so to speak, ideas as twigs bear leaves. Such an image may pass as a figure of speech. But how does a system of motor tendencies, existent only in succession, and neither one cognizant of any other, form a 'profounder' explanation of our power of mental synthesis than does the 'association of ideas,' even when the latter is atomistically understood? The motor tendencies can *realize* the whole synthesis from moment to moment no more than the loose and separate ideas can; but this realization of the whole synthesis at once, this modification of one element by another of the system, this cognizance of past and future along with present, is just the conscious phenomenon

to be explained. I must say that M. Godfernaux's notion of muscular synthesis, so far from shedding clearness on the subject, is itself in need of clearing up.

But in these general matters a second edition may bring improvement to the book, so I prefer not to criticise, but to notice some of the really luminous suggestions of detail which it contains. Its author begins by studying the alterations of the balance between intellect and feeling that occur in various insanities. His studies of mania and melancholy here are more ingenious (and certainly in some points truer) than anything that I have read. Mania is an incoherence due to the lack of any systematic purpose in the thinking. Systematic purposes are one by one introduced into the mind by the process of education. A mother, for example, teaches her child to repeat a fable of La Fontaine by giving, if she be a competent teacher, a unity of interest to the successive words, which thus grow into a system, disconnected from the thousand irrelevant perceptions, auditory, visual, tactile, etc., by which they were accompanied in the learning. This system is essentially one of tendencies to vocal reaction, regulated at first by other tendencies, those of attention to the mother's appeals, but afterwards coherent on their own account. Any tendency which later may prompt the child to think of the fable will bring the latter up synthetically, or as a whole, and the irrelevant perceptions will not revive along with it. Observations of exalted memory in hypnotism, etc., show, however, that the irrelevant perceptions can persist subconsciously, and may consequently be liable to recall when the subject's interest in the fable as a synthetic object no longer exists as an inhibiting force. Now in mania all persistent purposes and interests on the part of the subject have temporarily lapsed, and what is the consequence? Inhibitions cease as well as tendencies to constructive synthesis; all possible ideas acquire an equal value for associative purposes and an equal susceptibility to revival; and thus we find explained, along with the 'objectivity' of the maniac, and his absence of definite interests and pursuits, his incoherence, and the great copiousness of his mind in single ideas and unexpected turns. As in mania we have a morbid exaggeration of what may be called elementary objective thought, following on the suppression of emotion; in melancholy and ecstasy, in turn, we have morbid exaggerations of emotion and a concomitant suppression of thought. One subjective interest, that of the patient's personal welfare, becomes supreme, and inhibits all the usual perceptions and associations by the fixity with which the ideas connected with this interest agglutinate themselves together. In the extreme degree of this expulsion of thought from the mind by feeling, we find what is called stupor,

a sort of direct perception of the self and naught besides, with no play of ideas whatever. In all this it is immaterial whether the invasive flood of feeling be of a depressive or of a joyous kind. Melancholy and ecstasy agree in both inhibiting thought's flow.

In normal life M. Godfernaux shows how we oscillate between moods of 'exaltation' (which is a mania-like condition), of 'optimism' (which is an ecstatic condition with 'rarefaction' of ideas), and of depression, in either its melancholy or its hypochondriac form. He enforces in a somewhat new way the doctrine that impulsive tendencies with a definite motor outlet allow a minimum of consciousness, and that conscious feeling and thought are both of them results of inhibition of motor-discharge. Throughout the volume he brings out the close implication with each other of the motor and the conscious life, and makes one feel vividly once more how large a field offers itself here for psychology to make new paths in. And although his own clearing of some of the paths cannot be regarded as definitively complete and satisfactory, it must be said that his book is the work of a genuinely original analyst and observer, from whose continued contribution to psychology we have evidently much to hope in the future. W. J.

Materials for the Study of Variation treated with especial regard to Discontinuity in the Origin of Species. WILLIAM BATESON. London and New York, Macmillan & Co., 1894. Pp. xv + 597.

This important work will probably set up in biology as vigorous a wave of observation and discussion as that which Weismann's works have occasioned. Darwinism in the stricter sense (to which the word Wallaceism might perhaps better be applied) is synonymous with the doctrine that the discrete differences between existing species are due to the summation, pursued through successive generations, of numerous small variations in the same direction, of which variations the intermediary degrees have become extinct, leaving 'chasms,' larger or smaller, between the extremes. The discontinuously-varying species thus formed nevertheless inhabit environments of which the influential factors, such as warmth, altitude, depth of water, salinity, etc., vary continuously; and it is hard to understand why, if so many intermediary steps once existed, they should so generally have perished in the struggle for life. Did they ever exist? May not some of the original variations have been more abrupt and discontinuous than Wallace and Darwin have supposed? This is Mr. Bateson's question, and most of his book consists of a catalogue of such cases as he has been able to find in every branch of the animal kingdom, of discontinuous variation. The cases are numbered from 1 to 886; and when I say that many of

them are typical and carry numerous references under them to their similars reported elsewhere in the literature, some idea of Mr. Bateson's industry, patience, and minuteness may be conceived. The details are not for the general reader, nor will critical comments on matter so morphological be expected in a psychological review. But the general thesis of discontinuity in variation is as important in psychogeny as in any other branch of the theory of evolution, so a brief word about Mr. Bateson's results cannot be out of place here.

He of course does not pretend to deny continuous variation, or the accumulation of small steps by selection. What he denies, or doubts, is that such variation and selection can by themselves be responsible for the entire diversity of the animal kingdom as now found. More often than is supposed, he thinks, the new variety must have been a sudden 'sport.' He mistrusts the explanation of so many of the small details of structure by survival of the fittest. Species are only approximately adapted to their circumstances, and live as much in spite, as by virtue of what they are. Dull color may protect a moth, but the particular benefit of one pattern of dulness over the closely-similar pattern of the next species is almost never clear to the inquirer. Colorings are due to the correlated variation of enormous numbers of scales, hairs, or feathers. Have these varied piecemeal and been selected by successive steps, or have they varied simultaneously as a system? The simultaneous variation of all the similar elements of a system of tissues must be admitted as a possibility; and the result of Mr. Bateson's collation of facts is to make it a probability. For instance, in the matter of color he gives a heap of cases where the differently-colored varieties of a species tend to fall into a few discrete groups, and rightly says that this suggests much rather the presence of definite steps of chemical instability in the coloring matter than an environmental destruction of once-existent intermediary tints whose harmfulness we cannot guess. Similarly of the variations of size in an organ. The forceps of earwigs and the horns of beetles are measured, percented, and plotted on curves by our author to show that certain species are 'dimorphic,' and have two sizes of greatest frequency round which the other sizes cluster. These two discontinuous sizes seem therefore to represent conditions of least morphological resistance or strain under the specific conditions. The current explanation of such facts by reversion is, as Mr. Bateson points out, a conjecture without proof.—From such 'substantive' variations, as he calls them, as these, he passes to what he names 'meristic' variations, or variations affecting serially-repeated segments or appendages of the body, and shows not only that new segments may suddenly appear with all their parts complete, but that the same parts

of segments may similarly vary throughout a whole series. The bulk of the book is taken up with these meristic variations. All the divisions of the animal, and some of the vegetal, kingdom are passed in review successively for illustrations. The outcome is the exhibition of an immense amount of abrupt variation, none of which I will retail to the reader. Some of it is of the sort on which new species are founded, as where vertebræ, teeth, tarsal joints, dermal plates, annular segments, etc., appear in unusual number; but much of it consists of what can only be called monstrosity, as extra legs, horns, claws, fingers, or, speaking generally, of organs of a useful type growing where they should not be. It must be said on the whole that the bearing of the cases which Mr. Bateson has collected with such admirable diligence is more directly physiological than it is phylogenetic. So many of the odd things he shows us are mere extra copies of organs already adaptively evolved, that radical selectionists will say they throw no light on the original process of the evolving, but only prove the existence, when an organ has once been evolved, of a tendency to what Prof. Owen might have called its irrelative repetition. It seems probable that a hard fight may be waged on this point. Meanwhile Mr. Bateson certainly has, especially by his elaborate discussion of extra limbs and parts of limbs, given a considerable push to the more mechanical manner of looking upon organic growth. The 'major' symmetry which, from the first bisection of the ovum, dominates the forms of so much of the animal kingdom, suggests strongly the influence of mechanical forces, especially when we consider the not infrequent cases of duplication of median organs (as the uterus) and the fusion of symmetrical ones (as the eyes). But Mr. Bateson proves also the existence of a tendency to 'minor' symmetry in the limbs and other appendages. In vertebrates, e.g., the phenomenon of extra digits is not rare. In only one case, however, that of a monkey, out of the countless numbers studied by Mr. B. in museums and printed records, do the redundant parts add themselves to the normal ones in successive parallel order. In all the others they seem added in mirror-order, with an axis of symmetry somewhere in the midst—in fact the name of 'double-hand' has been given to some of these cases. The addition thus appears as a unit, with a certain tendency to relate its parts; and a force must be admitted which may at all events lead to the production of total organs at once, whatever effect such force may be found to have in the modification of species by descent.

Amongst the by-products of Mr. Bateson's investigations we find his disbelief in the current doctrine that domestic animals, taken generally, are more variable than wild ones, and his almost equal disbelief

in the rule that a sporadic variation must be quickly swamped. It seems to be a matter of the constitution of the particular animal considered.

As regards psychology, it is clear that the triumph of views like Mr. Bateson's will strengthen the hands of the anti-associationists, and in general of those who have contended for an autogenous origin of certain human faculties, of certain instincts and tastes, for example, or of conscience, the higher reason, and the religious sense. The book is a masterly production, and unquestionably inaugurates a new department of research. W. J.

Cock-Lane and Common Sense. ANDREW LANG. London and New York, Longmans, 1894.

Die Entdeckung der Seele durch die Geheimwissenschaften. CARL DU PREL. Leipzig, Günther, 1894. Pp. 258.

Mr. Lang has the memory of a bookworm and the pen of a *fin-de-siècle* journalist. The result here is a very curious compound of erudition and flippancy, in which the author drags us up and down all the ages of history and to and from all the ends of the earth, in order to make us feel the improbability that clairvoyant trances, 'levitations,' knockings, scratchings, and other noises, stone-throwings, movements of furniture, ghostly apparitions and the like, which *semper et ubique* have been alleged forms of experience, should be due to nothing but an original folk-lore tradition perpetuated and copied by sporadically-recurring fraud. From these persistent and apparently natural types of phenomenon he distinguishes genuine folk-lore beliefs like that in brownies, fairies, and the witches' sabbath, which are much less omnipresent in human history. He makes very merry over the unexacting rules by which 'Science' has hitherto held herself bound in giving explanation of these narratives, and finally he himself—declines to conclude! In all this his state of mind is the pattern and exemplar of what at all times has been that of the 'man of the world.' To be sure Mr. Lang, when his learning is considered, is a very rare man of the world. But he has the worldly lack of reverence even for 'Science,' as well as the worldly bias for fair play and relish for what he calls 'sportsmanlike' treatment of a subject. He has the worldly suspicion that 'where there is smoke there is fire,' but also the worldly dislike to push a thing too far, the worldly reluctance to stand committed and responsible, and the worldly love of keeping some thrilling mystery perpetually open to play with. So his book baffles the reader as the subject has baffled the author; and the most one can say of it is that it is the typ-

ical expression of a state of mind that is now common enough. As a skirmisher in the cause of 'psychical research' it will probably be effective; but it should have had an index, to make it useful to the more serious student of the sort of material which it contains.

If Mr. Lang feels baffled by his facts, not so does Baron du Prel. This writer has a *Schlagfertigkeit* at explanation quite equal to the great range of his learning, but the present work is a poor one by which to judge him on the theoretic side. The reasonings on which his theory of the 'transcendental Subject' is grounded are more fully given in his other works. The present one rather takes the theory ready-made, and in a number of chapters gives illustrations of its way of working in such things as emotional and æsthetic expression, somnambulism, thought-transference, clairvoyance, premonition, automatic writing, and speech in foreign tongues. The book is in fact a collection of distinct essays with the transcendental Subject as their nucleus. Our conscious intelligence or Ego, according to Du Prel, is only a partial manifestation of our soul, dependent on the brain and the senses. It has its roots in an extension of the same soul, which in addition to possessing the non-sensuous powers of cognition manifested in trance-states, etc., is the architect of the body and guider of its organic processes, and consequently the original moulder of the brain and senses themselves. This transcendental Subject is an individual entity, and so far Dr. Du Prel is not a Monist; though if we ascend to the substance of the world he admits that the various transcendental subjects may be englobed in the ultimate unity. In all this his hypothesis is more positive and elaborate than Mr. Myers's doctrine of the subliminal consciousness, and less elaborate than the 'theosophic' theory of personality. In the ordinary dream-phenomenon of conversing with an external interlocutor whom on waking we recognize to be our own creation, he finds an analogue of the relations of the normal or sensuous consciousness to the transcendental Self. After the great awakening we may find our sense-life similarly reabsorbed into the wider transcendental unity. That the dream-life plays a great part in the establishment of our author's ideas, those acquainted with his Philosophy of Mysticism will remember. In the present book he explains 'premonitions' (as, for example, the giving-up of one's passage in a steamer on account of a sense of impending evil) as due to emotional vestiges in the waking consciousness of clairvoyant prophetic dreams whose sensible details have been forgotten. The slenderness of the clues which Baron du Prel is not afraid to follow is shown in the first essay, 'On the psychic activity of the Artist,' of which the thesis, briefly given, is that the power that produces works of genius is the same supersensuous

Subject that makes the artist's own organism. The proof of this is that while talent copies nature, genius does not copy but produces works co-ordinate with nature, lending soul and life to the bodily things it represents, as in the personifications of nature in lyric poetry ; and, as in the dramatic and pictorial expression of the emotions, giving body and object to the thoughts of the soul. The same soul that drew the gestures in Leonardo's Last Supper, etc., prompted those gestures in Leonardo's person, and organized Leonardo's nervous system for their execution.

The range of our author's anecdotes is very great, and his choice of them absolutely uncritical. He appears to hold for true anything which any one may ever have reported, the publications of the Psychical Research Society being almost the only source not drawn upon in his pages. Add to this his unchartered freedom of theorizing, and the result is of course completely 'unsatisfactory,' although the book remains 'suggestive' enough. But in the present era of anarchy in these outlawed matters no one can be punished for any special sort of unsatisfactoriness in which he may prefer to indulge, so I say no more. Nevertheless between Mr. Lang's facility in leaving things unsettled, and Baron du Prel's facility in concluding them, it seems as if a better path might be found. Might not the earnest temper of science be combined somewhere with Du Prel's learning and the power of doubt of Lang? So far Mr. Myers's papers on the 'Subliminal Self' seem to have kept nearest to this ideal ; and both Lang's and Du Prel's books set off by contrast the superiority of his work. W. J.

THE NERVOUS SYSTEM.

Die Nervenzelle bei der Geburt und beim Tode an Alterschwäche. C. F. HODGE. Anatomischer Anzeiger, Bd. IX. No. 23. Journ. of Physiol., vol. XVII. Nos. 1 and 2.

These studies have been made on men and bees. That the active tissues of the body represented by the glands, nerves, muscles, and blood should exhibit changes in their structure due to old age was long ago probable, from what was known of cell-activity. One after another such changes have been demonstrated and the present paper shows the differences in certain parts of the nervous system of very old bees as compared with those that have just hatched from the pupa; and between the cells of a child at birth and those of an old man dying at the age of 92 years. The differences are illustrated by figures. When in the latter case the spinal ganglion-cells of the man were compared with those from the child, taken as a standard, it was found

that in the senile cells the volume of the nucleus was reduced to 64 per cent, that it was irregular in outline and shape, that the nucleolus was visible in but one tenth as many cells as in the child, and that while in the latter the cells were not at all pigmented, in the old man all of them had pigment, and in two thirds of them it was abundant. In the cerebellum it appeared that some of the Purkinje's cells were shrunken and that some perhaps had entirely disappeared. In the cerebrum no differences were determined, but in this locality the inquiry was not extensive. In the young bees the nerve-cells are smaller than in the aged ones (antennary ganglion), they have a large nucleus surrounded by a thin layer of cytoplasm, and are absolutely more numerous in the young than in the very old, in the proportion of 2.9 cells to 1. The senile cells have shrunken nuclei and the cytoplasm reduced to shreds, separated by large vacuoles. The gross changes in these cells for both men and bees are similar to changes found in the fatigued nerve-cells (bees and cats), but the absence of increased granulation and a deeper staining of the nuclei indicates that the chemical constitution of the cell has altered in the process of growing old. There are grounds for a close analogy between fatigue and senescence, and the differences in the nerve-elements doubtless depend on the fact that while fatigue is accompanied by the consumption of the stored materials in the cytoplasm, old age is characterized by a diminution not only of the stored materials themselves but in the power of restoration.

A Microscopical Study of the Nerve-cell during Electrical Stimulation. C. F. HODGE. Journal of Morphology, vol. IX. No. 3, 1894.

The changes in the nerve-cell which Hodge has been able to observe after electrical stimulation of the sensory nerves in the frog and cat; as the result of diurnal fatigue in birds and bees, and as an expression of old age in bees and man, have led him to attempt the direct observation of nerve-cells while they were being stimulated. The cells employed were those of the spinal and sympathetic ganglia of the frog. The method of observation consisted in removing symmetrical ganglia from the same animal and placing each ganglion on the stage of a microscope where its further changes could be followed. Both were enclosed in chambers and irrigated by a nutrient fluid. On one slide, however, wires had been laid, so that the cells there located could receive electrical stimulation. The two specimens were then examined from time to time and the nuclei in which the principal changes occurred were drawn and measured at regular intervals. The manipulation of such experiments, which were carried on anywhere from fifteen minutes to six days, was extremely difficult, and hence

many variations were observed, doubtless due to lack of identity in the experimental conditions. If the stimulus is not strong enough to either paralyze or kill the cell in a short time, those changes which have been described as characteristic of fatigue were seen to occur in the nucleus and could be followed step by step. The result of direct observation gives, therefore, a full confirmation of the previous conclusions. At the same time a number of new and interesting facts were incidentally observed. The reactions of the cells were in general similar at all seasons of the year. In some cases (2) neither nuclei nor nucleoli of the nerve-cells could be seen, this peculiarity occurring both in winter and summer frogs. On stimulation the nuclei became faintly visible, but no nucleoli were observed. In some instances the cytoplasm became lighter and clearer and oil-droplets tended to disappear as the result of stimulation, but in the living cell vacuolation was not evident.

During stimulation some of the granules in the nucleolus were extruded into the nucleus, and very slight changes in the constitution of the nutrient fluid used for irrigation produced startling results. The addition of .1 per cent of potassium tartrate to the solution of sodium chloride and calcium phosphate caused movements in the nucleolus; movements which were apparently amœboid. Under this treatment the nucleolus of the control cells only changed slightly in size, whereas the fragments of the stimulated nucleolus had all disappeared in thirty minutes. These facts are sufficient to show that the physiology of the nerve-cell is a suitable field for further study.

Beiträge zur Kenntniss des Reichthums der Grosshirnrinde des Menschen an Markhaltigen Nervenfasern. THEODOR KAES. Archiv f. Psychiatrie, Bd. XXV. Heft 3.

Ueber die Markhaltigen Nervenfasern in der Grosshirnrinde des Menschen. THEODOR KAES. Neurologisches Centralblatt, No. 11, 1894.

The former of these papers is the more elaborate and the latter contains a corroboration of the observations there described. The investigation concerns the differences in the thickness of the entire cortex and its several layers in a youth of 18 as compared with a man of 38 years, attention being given to the several strata of tangential fibres and especially to those in the outer half of the cortex. The material consisted of both hemispheres of the youth and the right hemisphere of the man. A large number of samples from every portion were taken, and the sections were stained by Wolter's method, which renders the

medullated fibres black on a yellow ground. A naked-eye comparison of the sections thus prepared showed that nearly twice as many yellow, and one half as many yellow-gray and gray sections belonged to the youth, as to the man. Thus in a general way the cortex of the youth was found less blackened and consequently less well medullated than that of the man. The total thickness of the cortex was greatest in the man. The measurements taken at the summits of the gyri and the sides and bottom of the sulci show that the most marked increase in thickness occurred at the summit of the gyri. The convex surface of the brain underwent least increase, while the ventral and median surfaces showed the greatest change, that in the latter being most marked. The several fibre-layers do not exactly follow these changes in the total thickness; the outer layers of tangential fibres being most increased in thickness on the convex surface of the hemisphere where the cortex has gained the least in total depth. The most marked development of fibres in the man was found in the motor regions on the convex surface and in the temporal and occipital lobes about the centres for hearing and sight respectively. Particularly poor in fibres are the interior portion of the frontal lobes and the insula. Interesting is the observation that neighboring samples of the cortex may be quite differently developed. These facts, taken with observations by Vulpius along the same line, show a long-continued growth in the human cortex, a growth which quite escapes detection in the gross weighing of the encephalon and its parts, and yet one which certainly is in progress during the process of formal education, and can perhaps be influenced for good or ill by training. In itself the increasing medullation of the fibres is taken to mean a better organization of cortex by an increase in the functional connection between the cells, there slowly developing.

H. H. D.

The Microscopical Examination of the Human Brain. EDWIN GOODALL. London, Baillière, Tindall & Cox, 1894. Pp. 186.

This little work is intended to give us a complete review of the methods of microscopical examination of the brain.

G. treats successfully the fresh method, the injection of cerebral blood-vessels, the hardening methods, fixation methods, imbedding, infiltration and section cutting, the staining methods, hardening combined with staining, the clearing agents, and mounting media. He then adds an outline of the general plan of procedure in microscopical examination of the brain. The appendix contains the methods for museum specimens, and a valuable schedule of the equipment of a laboratory such as is needed in hospitals. A scheme for the *post-mortem* record

is added, but no description of any special method to be followed in dissection.

Like most of the books of this kind it offers nothing from the point of view of a rational chemical or empirical explanation of the course of reasoning that led to the adoption of the rules given. We have little more than a collection of prescriptions before us, with condensed in struction for their use.

The compilation of methods is very complete. The little work will not fail to have an important place in every neurological laboratory. It is to be hoped that the author will furnish soon a second part, containing the methods for examining spinal cord and nerves.

ADOLF MEYER.

EASTERN ILLINOIS HOSPITAL FOR THE INSANE.

IDIOCY AND IMBECILITY.

L'idiotie et l'imbécilité au point de vue nosographique. PAUL SOLLIER. Archives de Neurologie, vol. XXVII. 33–38.

Recherches cliniques et thérapeutiques sur l'épilepsie, l'hysterie et l'idiotie, etc. Compte rendu de 1892, vol. XIII. BOURNEVILLE. Paris, Bureaux du Progrès Medical, 1893. Pp. cxii + 364.

L'idiotie, hérédité et dégénéréscence mentale, psychologie et education de l'idiot. JULES VOISIN. Paris, F. Alcan, 1893. Pp. 295.

Zur Ätiologie der Idiotie. HERMANN PIPER. Berlin, Fischer, 1893. Pp. 207.

Of late years the literature of idiocy has concerned itself chiefly with questions of classification, the differences of the different authors arising as usual from their various points of view. The first and perhaps the most important question at issue is whether idiocy is a condition distinct from imbecility or whether they are both merely degrees of one and the same affection. The latter is the view held by most writers in the past and at the present day, and it is to combat this that M. Sollier presents the paper mentioned above, supporting with clinical and pathological evidence a position he has already taken on psychological grounds in his well-known book 'Psychologie de l'idiot et de l'imbécile.' * In that work M. Sollier argued that idiocy and imbecility are, psychologically and socially, two distinct states, being united by a single common factor, viz., that their intelligence is inferior to the normal. Further, that if the idiot is to be considered as an

* Bibl. de philosophie contemporaine. Paris, Alcan, 1891.

inferior individual, incapable of living independently in society, and if the epithet 'extra-social' applies to him for that reason, then that of 'anti-social' should be given to the imbecile, an individual who is not only incapable and useless but even a menace and dangerous to society. In the paper before us, as has been said, M. Sollier approaches the question from its medical side and, to be brief, lays down the proposition that idiocy is not a disease in itself but a symptom; a position that would be accepted, perhaps, by many of his opponents without demurring, but with the addition that if idiocy be only a symptom, then imbecility is but a lighter degree of that same symptom: but here M. Sollier vigorously objects, holding that imbecility is a distinct disease, thus establishing at once a radical difference between the two states.

In support of his position he urges that all idiots present cerebral lesions, while imbeciles do not. One could wish that M. Sollier's evidence were more exact and conclusive, especially regarding the point last made; as it is, his argument while clever is not convincing.

Among the chief opponents of the last writer must be reckoned Bourneville, who, however, in the excellent record before us of the year's work in the Bicêtre does not touch on theoretical questions, but confines himself to clinical and pathological descriptions of the cases under his charge, a record that might well serve as a model to other alienists in similar positions.

Approaching the question of classification of different forms or degrees of idiocy, we find here also a wide divergence among authorities according to their basis of classification, e.g. etiology, symptomatology, or pathological anatomy. Voisin, like most authors, making no radical distinction between idiocy and imbecility, presents four categories depending on the origin or degree of development of the mental weakness:

1. Idiocy, complete and absolute, may be congenital or acquired. This form is incurable.

2. Idiocy, incomplete, congenital, or acquired; capable of amelioration.

3. Imbecility, congenital or acquired; characterized by the rudimentary presence of all the intellectual faculties; by the degeneration and instability of these faculties.

4. Mental weakness (*débilité mentale*), characterized by feebleness or lack of equilibrium of the faculties. Voisin further finds two degrees in his first class, viz., those idiots who possess the instinct of '*conservation*,' and those in whom that instinct does not exist.

This classification is open to perhaps fewer objections than any we

have yet seen, and is one of many good points in a book that as a whole deserves the warmest praise as a clear and succinct description of the field. The author's account of the special and general sensibility of idiots may be mentioned as particularly suggestive, as well as the treatise at the close of the book on the best methods of education for this class of unfortunates.

Of quite another type is Herr Piper's report of his researches in the etiology of idiocy. Fortunate in his material, he has made an important contribution to the statistics of the subject. We may note in passing his classification, not altogether happy, depending upon the presence or absence of convulsions, and distinguishing further a congenital and an acquired form of idiocy. Of 416 cases under his charge 310 were congenital and 106 acquired, while 70% are described as having convulsions. Of all his patients 32% were first-born children. As to the causal circumstances in congenital idiocy: in 23% there was a history of pulmonary tuberculosis in the parents; in 14% of mental disease of one kind or another in parents or immediate relatives; while in 17% no cause could be advanced. In 10% of the cases the father was a drunkard. Consanguinity of parents could only be shown in 3%.

In acquired idiocy the infectious fevers are by far the most fruitful cause. The proportion of idiotic boys to girls was nearly two to one (276 : 140), a difference that could not be due to accidental causes alone. Open to error as all such asylum records are, Herr Piper has nevertheless presented probably the most important single statistical study in this subject of the last decade.

LIVINGSTON FARRAND.

COLUMBIA COLLEGE.

THE PERCEPTION OF TIME.

Beiträge zur Psychologie d. Zeitsinns. ERNST MEUMANN. Philos. Stud., VIII. 431–509, 1892; IX. 264–306, 1893. To be continued.

Untersuchungen zur Psychologie und Æsthetik des Rythmus. ERNST MEUMANN. Philos. Stud., X. 249–322, 393–430, 1894. To be continued.

These two articles, already filling nearly 250 pages and running into three years of publication, are neither of them yet completed. The first instalment (79 pages) is a review of contemporary workers in the field chosen: Torkelson, Münsterberg, Schumann, Nichols. M. commends Torkelson for raising the still open question in time-experimentation: How shall we compare judgments taken from different stages of improvement and practice? M. finds Münsterberg's measure-

ments careless and shallow. Rejecting the latter's 'muscle' hypothesis, M. advocates, and I think rightly, that every psychic process is a competent basis of time-measurement. Some processes serve better than others—sound better than muscle, and muscle better than eye—"Münsterberg's doctrine being but an exaggeration of this fact." The remainder of Part I is devoted mainly to Schumann. Schumann's apparatus is mechanically perfect, but too difficult of manipulation to admit of adequate range of experimentation. Schumann's work must have contained errors from improper use of telephone, mercury contact, etc., and with his method of computing results M. finds much fault. Hence Schumann's sole products, as judged by M., are "obscurities, indecisive conjectures, and everywhere gaps patched up with great trouble and gratuitous assumptions." Relative to Schumann's main theory, that waiting and surprise are the crucial elements in time-judgment, M. declares these latter to be merely disturbance-phenomena which accompany uncertain judgment, and which vanish as accuracy is reached. The temper of M.'s criticisms is exampled by the following: "Schumann either must hold to the absurdity that we compare surprise and waiting, or he must admit that with judgments based upon (*stützt*) waiting and surprise there can be no talk of comparison. Upon these logical grounds, therefore, the whole theory (of Schumann) falls immediately to complete nonsense." My own work on Time—that presented for my doctorate—M. disposes of in a single page, wherefore I esteem his good sense, I now holding it to be of no value, and wishing it had never been written.

The above, then, is 'The Present State of the Time Problem,' as reported by M. Nothing but misconception, carelessness, and nonsense! It is difficult to do justice to such an author. Throughout we are grateful for his extremely valuable opinion. The temper displayed toward Münsterberg and Schumann is, however, deeply to be regretted, since it robs his own words of judicial weight. We are inclined to believe that M. would have made his papers stand among the best in psychological literature, had he burdened himself less with unworthy feelings, and given his great intelligence more appreciative scope.

We now come to the second instalment, containing an account of M.'s own experiments done in Prof. Wundt's laboratory at Leipzig. These 43 pages are the most valuable in the parts yet printed. M. first investigated as to whether an insistently-repeated perception of small difference results in inability to perceive any difference, or inability to perceive no difference—a point too nice, it would seem, for M. to have made entirely clear. In 1891 M. began to study the curve of sensibility for empty intervals (.05–8. s). The experiments were aban-

doned because of three sources of error : inclination to give set answers ; power of subject to make small differences seem 'longer' or 'shorter' at will ; tendency of subject to imagine beats or rhythms in series of sounds, which tendency made the intervals seem of variable lengths. These errors led M. to believe that we judge short intervals (below 5 s) in an entirely different manner from long ones ; that with the former, the succession of strokes is the object of attention, while with the latter the attention is directed to the conscious processes lying between the strokes. M. holds that the time-content comes to cognition in quite another form in the one case than in the other ; he says we judge a rhythm as a whole, and in consequence, time-memory of the preceding or repeated interval plays no part in judging short intervals. These are the most important suggestions contributed in M.'s papers, and are likely to prove significant in future experimentation upon the time-problem. After the above M. turns to the study of rhythms. He gave his subjects series of 50 sharp sounds followed by 50 lighter ones. The intervals bounded by the heavier strokes seemed shorter. M. explains by saying that heavy strokes fuse, and make the series seem less continuous and therefore shorter than do dull or soft strokes. As an explanation this is surely surprising. M. found that a strong stroke, introduced into a series of weaker ones, made the interval preceding it appear shorter than the others, and the succeeding one longer. In the main experiments Wundt's new time-apparatus was set to give comparison of two intervals of equal lengths (about $\frac{1}{3}$ s) limited by strokes of different intensities or, in other words, to give rhythms of equal intervals but different accents. Studies were made with the accents in every possible combination for the limiting strokes. M. claims that throughout, the intervals bounded by the intenser strokes seemed the shorter. This opinion seems to be a little forced in such cases as those where a single accent falls on the second stroke (1 2′ 3), with the result that the interval between 2 and 3 seemed the shorter. M. is obliged to reason here that the unaccented 3, coming after the accented 2, seems abnormally weak, and so gives the second interval the appearance of being bounded by weak strokes. We fear that this is the old phantom-psychology—that the act of comparison, set up for explanation, is more of a mystery than the thing to be explained. M. modifies his experiments by placing hammers at unlike distances from the two ears. The gist of the whole work is that change of intensity has influence upon the time-judgment as well as does the length of the time-interval itself. The point raised is a good one, but we feel that M. has not entirely disposed of it.

The two instalments at present given us of the paper on Rhythm are

chiefly historical, and are valuable for their bibliography. In Chap. I. 'General Theories of Rhythm' are reviewed (Moritz, Scherer, Benecke, Darwin, Spencer, Schlegel, Schopenhauer, von Hartmann, Köslin, Lotze, Fechner, Herbart, Zimmermann, Mach, Horwicz, Wundt); Chap. II, General Theories from Musicians (Hauptmann, Westphal, Riemann); Chap. III, Rhythm of Spoken Verse (Minor, Paull, Brücke); Chap. IV, Beginnings of Experimental Investigations of Rhythm (Brücke, Hensen, Pipping, Boeke, Wagner and Vietor, Rousselot, Ebbinghaus, Müller and Schumann). In this last chapter are valuable descriptions of modern apparatus for investigating vocal sounds and speech.

M.'s literary style is one that does him the greatest injustice. What is said in 250 pages could be better stated in 50. The different topics are mixed up and strung along with such obscurity and confusion that only with the greatest patience can one discover the author's intended meaning. The work is scholarly, and on the whole the best that has yet appeared in this field. No one can read it without profit or without appreciating the tremendous zeal and patience with which it has been produced. Yet in these days of expectant search for inmost psychological truths one must be disappointed to find that Meumann's theory of time-psychology nowhere gets beyond the notion, that perception of time-content is an ultimate and irreducible fact; or at best gets no further than a disturbing suspicion that perhaps it is a process in some way based upon attention. We repeat that the experimental results are the most valuable part of the author's contributions, as perhaps was to be expected, coming as they do from a laboratory to which, at present, experimental psychology owes a greater total obligation than to any other.

HERBERT NICHOLS.

CAMBRIDGE.

EXPERIMENTAL.

Influence de l'age sur la mémoire immédiate. B. BOURDON. Rev. Philos. xix. 148–167. 1894.

Bourdon reports a series of experiments made upon one hundred and four Lycée pupils between the ages of eight and twenty. Series formed respectively of digits, letters, monosyllables, dissyllables and trisyllables are distinctly pronounced and the pupils required immediately to repeat them. As is usual in such experiments, precautions of various kinds are employed to prevent disturbances from rhythm, rhyme, trains of habitually associated ideas, etc., the aim being to secure for memorizing presentative elements which possess as nearly as possible equivalent tendencies to fresh associations.

Bourdon finds (1) that between the ages eight and fourteen the 'immediate' memory grows rapidly in power. From fourteen to twenty the growth is almost imperceptible. (2) Not more than six or eight digits can be memorized in this way. For words the limit is five or six. (3) Under the age of fourteen, digits are most easily memorized. After that age, no essential difference appears in the various series. (4) The results show an unquestionable (?) connection between the general intelligence, as estimated by the teachers, and the power of memory. (5) Memory of order as distinct from mere memory of the individual presentation is clearly shown. (6) Series in which repetitions occur seem more difficult to memorize than others.

Not to mention similarities to other investigations of recent date, Bourdon's work, both as regards purpose and method, is strikingly akin to the more extensive and, in most respects, more carefully executed work of T. L. Bolton,* yet there is not even a suggestion that the problem in hand has ever been attacked. Bolton agrees with point (2) and of course with (5). He disagrees with (3) and states (1) in a much more conservative way, besides reaching a number of other conclusions. It is thoroughly regrettable—to put it no more strongly—that experimentation of this kind should be undertaken with so little reference to what has already been accomplished.

JAMES R. ANGELL.

UNIVERSITY OF CHICAGO.

The Relation of the Interference to the Practice Effect of an Association. JOHN A. BERGSTRÖM. Am. Journ. of Psychology, vi. 433–442. 1894.

Mr. Bergström continues his study of the interference of different lines of association. He formulates his specific problem as the "relation of the interference to the practice effect of an association." Suppose that a given series of stimuli, as 1, 2, 3 ... 8 is associated first with one set of psychic processes, *a*, *b*, *c* ... *h*, and later with another set, *i*, *j*, *k* ... *p*; and then let Series I. (the association of 1, 2, 3, etc., with *a*, *b*, *c*, etc.) be repeated: what, if any, has been the disturbing effect of the intervening series?

Mr. Bergström's experiments were directed to the determination of one of the three possible hypotheses: (1) that the practice and interference effects simply cancel each other; (2) that both influences remain in some constant relation; (3) that this relation is an occasional and variable one. The experiments are on the general plan of those already

* *Memory in Children*, Amer. Jour. Psych., IV. 362.

reported;* the material consists of a pack of eighty cards, containing outline-pictures, sorted into ten piles in such a way that ten arrangements are possible, and of packs of comparison-cards containing different pictures or printed words.

Mr. Bergström's conclusion, in opposition to that of Münsterberg† and of Müller and Schumann,‡ in experiments to which reference is made, is "that under the simple conditions of this experiment, the interference-effect of an association bears a constant relation to the practice-effect, and is in fact equivalent to it."

For details of the work Mr. Bergström's paper must be consulted, for it is too skilfully condensed to lend itself readily to further abbreviation.

MARY WHITON CALKINS.

WELLESLEY COLLEGE.

Les actions d'arrêt dans les phénomènes de la parole. A. BINET and V. HENRI. Rev. Philos. XXXVII. 608–620, 1894.

Messrs. Binet and Henri describe some experiments on the time-measurement of speech, which have an important bearing on the subject of inhibition. They find that the time required to utter a syllable depends, in addition to its phonetic value, (1) on its position in the sentence (at the beginning, middle, or end), and (2) on its inflection, as marking the rhythm or meaning of the sentence. In pronouncing the numerals from one to ten, as the rate of speed was increased, the intervals between the numbers were diminished by about one half, and the numbers themselves by about one third, excepting the last, which remained the same length. When the numbers were grouped, the last number of a group was always appreciably longer. It would be interesting to repeat these experiments for English and German observers, and also to investigate the difference between vowel and consonant endings.—In another series the subject uttered the numerals rapidly, and stopped at a signal: this was compared with a series in which a sound was prolonged till a given signal, when another was uttered in its place. In the latter case the time needed was substantially the same as the reaction-time for speech (220–260σ for the numerals), while the time for *arresting* a sound was considerably longer (averaging 340σ), and depended on the phase of utterance, or of interval between syllables, in which the signal was given.

An investigation of the effect of speech on respiration indicated a slight rise in the curve at the outset, while the utterance itself was accompanied by exhalation. We would suggest that the former effect

* Am. Journ. Psych., v. 3. † Beiträge, Heft IV.
‡ Zeitschr. f. Psych. u. Phys. d. Sinnesorgane, Bd. VI. 2 & 3, p. 173.

may be due to the muscles of the diaphragm contracting in order to expel the air forcibly, as speech requires, rather than to any actual inhalation.

H. C. WARREN.
PRINCETON.

Beiträge zur Theorie der psychischen Analyse. A. MEINONG. Zeitsch. für Psychol., VI. 340–385, 417–455.

These subtle and elaborate studies, occasioned by an article of H. Cornelius (Viert. f. wiss. Phil., XVI and XVII), treat of a succession of more or less connected questions respecting the presuppositions, difficulties, range and essential nature of the common analytic process by which some element in a complex mental presentation is 'brought out' or 'emerges.' There are two all-important presuppositions of ordinary thought in the matter which need to be tested. First, as to the presupposition of *active* analysis: is it really analysis, a disclosure of parts that were present before, or does the analytic process *change* the content and *create* the new-found elements? The uncritical person holds the former, but he is thinking of the permanence of the stimuli. Memory, recognition, and, in the clearest cases, comparison of the sensation before and after analysis, testify also (so far as they can) to the pre-existence of the disclosed element. The question is decided when we inquire *how* a change of content could be brought about. It could only be because the stimuli of the elements successively disengaged by analysis are at first interfering with each other and producing fused effects; then the analytic process in turn isolates each of the different stimuli by paralyzing the others, and thus leaves it free to produce its proper result. But in the typical case of perception this seems impossible; for how could the analytic activity, while the sense-organs were intact and the paths of conduction open, exclude now this stimulus, now that, from entrance to the central organ? Besides, a fused content would present no distinct point of attack for the analytic operation. Further still, the probable *continuity* of the change brings special difficulties of its own. Thus we are thrown back on the other alternative, that the extricated elements were really there before and that analysis merely brings them under the light of the judging faculty, into the sphere of knowledge. Second, as to the presupposition of *passive* analysis, that is, of cases where some element 'stands out' of itself and is distinguished from the remainder. Is an element in a complex mental whole modified in content by conjunction with other elements? For example, where the conjunction establishes (*fundiert*) a new content, are the original components altered? No, the superinduced content leaves the first combining elements standing.

Taking the case of *timbre* and partial tones as typical, this may be set up as a general rule. Both, then, of the presuppositions of current reflective practice in this matter are justified.

The author goes on to maintain that analysis does not mean the taking of a thought to pieces; against Stumpf, that it is not 'the perception of plurality'; that it is not even knowledge or judgment, though it leads to them. The sphere of mental presentation is wider than that of judgment; judgment occupies the centre of consciousness, bare presentation the periphery. Every individual has but a limited judgment-capacity. Now what tends to bring a presentation within the judgment-circle; what are the factors of analytic attention? First, certain traits of the content: its intensity, certain kinds of quality, medium degrees of simplicity or complexity. Then traits not of the content: perception rather than imagination, Höffding's 'quality of familiarity,' intensity of the act of presentation,—and a factor entirely beyond the realm of *das Vorstellen*, what may be summed up as 'interest.' In virtue of these factors variously conjoined, each presentation has a certain general tendency to be drawn into the judgment-sphere which may be called its 'weight.' Weight is relative and competitive, not absolute. The content within the judgment-sphere when it is *discontinuous* and *articulated* may resolve itself into a plurality of lesser judgment-tracts, each marked by a special relation of its parts—another case of a superinduced content. In general psychic analysis may be defined as the contraction of the judgment-sphere by active augmentation of weight; or, since the process involves a contraction of the whole span of consciousness, it may be called *concentration*. Into the author's long-drawn discussion of the relation of analysis to the inner articulation of the judgment-sphere, into his appendix on the analysis of sequences, and the many minor but always elaborate ingenuities of the paper, we cannot follow him here.

The articles are marked by fairness, patience, and power of reasoning. But they furnish an almost classically finished instance of the way in which the preoccupation with categories and classifications and blank forms of argument—an over-elaborated machinery of method—may dull an investigator's mother-wit and coarsen into clumsiness his natural tact of treatment. The cogs and wheels, the grinding and pulverizing appliances set at work, revolving with the slow unavoidable creaking of the author's style, impress us only until we open the machine and find that there was nothing inside to be ground. The stuff to be dealt with had gradually sifted out by unobserved cracks as the jarring motion began. Herr Meinong's handling of the momentous question whether analysis finds or makes what it discovers, compared for in-

stance with Professor James's searching treatment of the same subject, is curiously unreal. His assumption of uniformity in the phenomena, his suggestions about the 'stimuli' and the difficulty of supposing them to be 'paralyzed' by the analytic activity, betray a strangely deficient sense of the complex delicacy and fluctuation of cerebral and mental life. His uncritical appeal to memory and comparison shows that elaborateness does not always mean care. And his conception of judgment and mere presentation as two irreducibly different grades of consciousness,—analysis consisting in dragging presentations before the judgment-seat,—congenial though it is to certain current German formulas, lies none the less exposed to the unanswered objections of those who regard 'judgment' as analyzable into terms of presentation.

D. S. MILLER.

BRYN MAWR COLLEGE.

THE PERSONAL AND SOCIAL SENSE.

The Meanings of Self: the Reality of Self. F. H. BRADLEY. Chaps. IX–X of the work 'Appearance and Reality.' London, Swan Sonnenschein & Co; New York, Macmillan & Co., 1893.

Mr. Bradley distinguishes eight meanings of 'Self.' He criticises them all and finds the following outcome. Nowhere is there any content of consciousness which is consistently and always called 'Self.' There is the anthropological self, a cross-section of consciousness, Hume's bundle of present states—which changes of course. There is the organized self of thought which proceeds upon ever new materials of organization. There is the quasi-permament self of memory and personal identity: but what is it that is permanent? There is the sentient self which finds itself subject to the contrasts, fluxes, relativities of feeling, and so on. The actual process of reflection on self is depicted by Mr. Bradley in an analysis which is wonderfully acute and obviously true; a landmark, I think, in the history of that enigma, the so-called 'rational subject.' He depicts a perpetual ego-play of content-elements over against one another in their relation of subject and object. At one time a certain arc in the trajectory of consciousness assumes the rôle of self over against another arc which it takes for its object. Then, at another time, the ego-section slides further around, so to speak. But however long you chase it, it is always part of the trajectory, part of the content—the ego is; and the object is another part. And the unity which contains the whole play, this is the only unity there is. It is a unity of feeling. Always, there is a *fundus* of feeling. This ego-play I find to be very truly described: try as one will to reflect on

self, he finds a content—that which is at that moment claiming to be the subject—setting itself over against another content and calling it 'me': and just as soon as one tries to find out what this subject-content is, he is able in a measure to do so; which means that that content has now taken the place of the object-content and so is no longer I, but has become me. And all the time there is a 'feeling' of the whole play, and of the background, as itself upholding the I and linking it into some kind of unity with the me.

The same analysis holds, says Bradley, also for the 'active' self—the self of volition and desire. It seems possible to turn upon any element in the self that desires, and desire *it* to be otherwise; that is, to treat it as a not-self upon which the action of the self desiring is to terminate. This leads to a subtle deduction of the sense of self-activity, which is shown to be due to change in content. For example, the I which desires finds in its object new elements of content fit to be included in the me, and by its expansion to include these elements it sets itself over against its former I-elements, thus converting them into objective me-elements. This expansion and shifting of content-elements through which certain constant I-elements are present—this is felt as self-activity. Even when the elements reached out after as fit for I-elements are not explicit,—i.e., when there is no explicit desire,—even then self-activity is felt. This is due, Bradley thinks, to the implicit presence of these elements already in the original I-content, but in such a way that the entire content as a group is inhibited by the explicit elements. The release of this inhibition is then felt as self-activity.

This deduction, it is clear, is capable of either a Herbartian or a Wundtian construction (see notice of Mackensie's paper), and it assumes, with both Herbart and Wundt, conscious self-activity beneath the threshold of explicit desire. With this assumption I do not agree. There is really no warrant for any such kind of self-activity. Consciousness bears witness, on the contrary, to a very clear aloofness of the I-content from both the members of the change of content taking place in a 'me' which is not the object of desire. Note the case of involuntary attention with its distractions, and the changes wrought in the me content by hypnotic suggestion: these have no feeling of self-activity.* Nor has the progress of a purely objective 'train of ideas.' And even in the instance of blind unratified impulse there is a feeling of 'run-away' in the machinery, of lack of self-implication, which is due not to the implicit presence of the elements which are explicitly present in desire, but to the weakness of another content which is ex-

* Cf. my volume on *Feeling and Will*, chap. XII. §§ 3–6.

plicitly desired. This latter content is inhibited and overcome, and the undesired takes place because of the *reverse outcome* of the same process as that of explicit desire. Mr. Bradley holds the necessity for some content-element ideally held for realization; but, in saying that after all it may be implicit, he seems to give up his analysis for the sake of accounting for a myth. The idea said to be implicit is really a part already of the old felt content—otherwise there is mere change, not activity—and this felt content maintains itself successfully against the ideal content. Hence the sense of incompleteness, disappointment, relative irresponsibility in such activities, i.e., as saying 'I will not consent,' and consenting. Put in symbols, there seems to be little difference here between Mr. Bradley's view and mine. But he, in fact, finds self-activity felt towards what is not desired; I rather find activity, largely not that of self, felt toward that which inhibits what is desired. In the concrete cases which psychology actually knows it makes a difference.

This analysis of self-activity—or any other which proceeds upon what Mr. Bradley calls 'the end in the beginning'—shows itself important in relation to the doctrine of imitative development worked out by recent writers. The object of desire, explicit or through habit implicit, is set up for realization. This is what I have called a 'copy for imitation' in my theory, such a copy as an imitative view of volition requires.* It seems then that this citadel of *actus purus*, this fount of originality and unrelated self-determination, is also capable of a natural construction. The pedagogical applications are very important. For 'self-activity' is talked of so freely nowadays as the goal of education—and so it is—that it is well to show that it is after all through imitation that the training process must proceed even in order to make our scholars inventive.

The other chapter of Bradley's—'The Reality of Self'—proceeds to show that in such a shifting self, constructed out of changing content, we have no right to find reality. It is appearance only. This involves the further doctrines of reality, appearance, change, etc., and is too far-reaching for further notice here.

Mr. Bradley's View of the Self. J. S. MACKENSIE. Mind, N. S. III, July 1894, pp. 304–335.

Mr. Mackensie gives an account of the chapter on the Self of Mr. Bradley's book, and criticises it on the score of certain omissions. He classifies Bradley's meanings of 'self' under four heads—the

* See also Royce's paper noticed further on.

'biological,' the 'psychological,' the 'sentient,' and the 'pathological' self—and claims that two other forms of 'self' must be added, called by him the 'epistemological' and the 'ontological' or 'ideal.' The epistemological or transcendental self is the form of the thought-process, the focus at which the variety of experience is brought to unity in thought. It is the Ego of the cogito and is not a matter of content; thus escaping Bradley's reduction of the various selves to particular constructions of content. In psychological terms, I suppose, this self is the function of apperception considered as unifying principle of thought. The other 'self' added by Mackensie is the 'ontological': again the formal principle of unity, but now considered as the unity of reality or completed system—the ideal unity of 'the completely intelligible for the completely intelligent.' Both these points are familiar to readers of Caird.

As to the matters of fact involved, I think Mr. Bradley is not well criticised. The question arises, how does 'form' come to consciousness? If not as content, we have to say, then not at all. But if not at all, then it must be itself a matter of thought-construction. For how can we say 'experience when thought has the form of unity' except by the use of judgment which must go back again to conscious-content for its matter. So the 'transcendental ego' becomes either the Kantian noumenon, or reduces itself to the 'sentient' self of Bradley, i.e., as I would put it, it is a matter of sentient or felt content over and above the presented content of which it is felt to be the form. In this shape it loses much of its mystery and is amenable to the same natural-history treatment as other facts of consciousness. And the 'ontological' or 'ideal' self is subject to the same kind of criticism. If there be no real *ego* discovered in the *cogito*, apart from the felt form of the *cognitum*, then we have no basis for an ideal *ego* discovered in an ideal *cogito* apart from *what we feel* the form of the ideal *cognitum* would be if we were able to apprehend it. Then presupposing absolute reality, with Bradley, the ideal ego will be an absolute sentient ego—an ego which feels its own perfect content.

I do not know whether Mr. Bradley would accept this bald argument to a conclusion near his own. It certainly is much briefer than his. And I am sure that Mr. Mackensie and his master would say, "not a word about 'reason'—which is a 'higher level' than intellect." But of the points still left in current idealism for Mr. Bradley's probing-knife of psychological analysis, this is the most inviting. I believe that reason is feeling, and its ideals are feeling—the onrush of habit and emotion in their own teleological movement beyond the constructions of intellect which they presuppose. This is its nature and history.

And it is Bradley's splendid service to have shown that reality is as much reality when felt as when judged—possibly more, if the pros and cons of the relation of feeling and thought to each other be duly weighed.

The External World and the Social Consciousness. JOSIAH ROYCE. Philos. Review, III. 513–545, Sept. 1894.

The thesis maintained by Prof. Royce in this interesting paper is this: "Social community is the differentia of our external world. . . . A child never gets his belief in our present objective world until he has first got his social consciousness." The arguments presented by the author in support of this view are of two kinds. He first show. that the ordinary so-called tests or criteria of externality are not valid or sufficient inasmuch as they omit the quality of *definiteness.* All things believed to be external are definite in place, dimensions, number, and movement. But what we really mean by definiteness is, when analyzed, *communicableness* to others; what I cannot express to my fellow and ratify together with him—that is not external, but internal. The notion of externality therefore proceeds upon the sense of social relationship or community. Apart from the question of proof, attention may be called to Prof. Royce's acute note on Renouvier's thesis, 'Whatever is must be determinate,' and to the use he makes of the sense of indefinite movement in after-images quoted from Fleischl. In what is said in this part of the paper we have, I think, a very original and interesting contribution to the theory of externality. It lacks, however, detailed criticism of the criteria usually named, i.e., resistance, regularity, involuntariness, etc., of the external world. I myself, for example, do not feel driven out of my view of the 'coefficient of external reality' * earlier worked out, even though the whole account of the social consciousness given by Prof. Royce be true. This appears in the general point of criticism made below.

In the second part of his paper, the author gives a summary of a theory of the rise of the social consciousness based upon the doctrine of imitation, i.e., a theory with which a recent paper by the present reviewer is in substantial agreement, as is intimated by Prof. Royce in an all-too-kindly reference.† The essence of the theory is that the child gets his material for the personality-sense from persons around him by imitation. So that his growing sense of self is constantly behind

* Baldwin, *Feeling and Will*, chap. VII. §§ 4, 5.

† *Mind*, Jan. 1894. On this topic an article by Prof. Royce on 'The Imitative Functions' in the *Century Magazine* for Mar. '94 should be read. It is to be followed by a short paper by the present writer in the same magazine.

his growing sense of others. This conclusion affords the additional argument that it is through this relationship that the antithesis between self and the external is discovered and the community made possible in which the external world finds its differentia.

The one criticism which I would venture to make upon this paper —as attractive in style as thoughtful in content—is that it neglects the phylogenetic point of view, the considerations from race-history. I think the element of social suggestion may be admitted to the full as Prof. Royce argues for it, and yet the conclusion not follow that the child never *would* get the notion of externality without it. No more would I say that the child never *would* get a notion of self without the imitative copying of others which we agree in emphasizing so strongly. Would not the hereditary impulses of thought and nervous action give an isolated babe a pretty good apology for an external world and a self? To say, 'yes, but not the same he has now,' is only to say that the new element is an addition. Certainly it is; but is there no essential moment in externality which is likely to be either there or not there to a child?

I think there is: something in the structure of the developed nervous system. The seeing of space itself seems to mean externality in presented objects: not not-self-ness, of course, but blank, *definite*, awayness—*da*-ness, so to speak. It is the property seen in the nervous projection of stimulations to the periphery. Little chickens seem to have a very respectably *definite* sense of *da*-ness, and this without comparing notes with one another or with the hen! Now this sense of projection may be the essence of external existence *vs.* internal—although the antithesis comes only later and largely by social development—and it may be that the elements even of personal suggestion which the child imitates already have it.* Indeed I think it can be shown that they have. It is on this basis that I give, in my 'coefficient of external reality,' the element which constitutes *this kind of objectivity*, and make the 'objective' stage first even in the child's knowledge of other persons.

An interesting speculation would arise if Prof. Royce should work out the social criterion in the phylogenetic sphere; by applying it, for example, to the quasi-social community of the different senses together—a test of externality strongly insisted upon sometimes. If so, I should ask him how it has come about that a single sense often so

*Cf. my paper on 'Personality Suggestion' in this REVIEW, I. p. 274, May 1894. I am pointing out in my book that there is a period of 'organic' bashfulness in the child's first year—showing a specialized nervous reaction in the presence of *persons*.

strenuously lies to us about externality, in the face of all sense and social testimony, that we have to lie to ourselves, almost, to keep back our belief in it. If it be because this function, say, of this sense is a part of habitual convention and former beliefs which are themselves guaranteed, then that illustrates what I should say was the case with each organism as a whole with reference to other organisms.

I cannot close this notice without mentioning the grace and impressiveness with which this paper was delivered as a lecture before the Princeton Philosophical Club. J. M. B.

The Oxford Meeting of the British Association. Reported in Nature, Aug. 9, 16, 23, 30, and Sept. 6, 1894. *Inaugural Address.* THE MARQUIS OF SALISBURY.

Attention may properly be called in this place to the reports in *Nature* of the recent meeting of the British Association. The address of the President and of the Presidents of the several sections are included in full, and abstracts are given of many other papers. There is no section for Psychology, but papers of considerable psychological importance are often presented before the section for Anthropology and elsewhere. It is impossible even to enumerate the many papers having some reference to psychology, but one of these, the inaugural address of the President, may be selected for notice.

Lord Salisbury departs from the usual precedent and surveys not our science but our ignorance. He selects three subjects for this purpose—the chemical elements, the ether, and the doctrine of natural selection. The chemical elements and especially the ether are difficulties in the way of a mechanical description of the physical world which are suggestive to the psychologist. In the case of natural selection Lord Salisbury seems to think the time limit set by physicists is one of the most serious difficulties. But if Lord Kelvin allow a hundred million years for the existence of organic life on the earth, and Prof. Tait ten million, the probable error is so considerable that the biologist may claim a thousand million years if he need them. As a matter of fact it is geology rather than biology that must seek reconciliation. A hundred million years is a long while, and under favorable conditions evolution may proceed rapidly—witness the mental and social development of man during the past three thousand years. But the point of special interest to the present writer in the address is the contradiction assumed by Lord Salisbury and by so many naturalists of natural selection and design. Prof. Weismann says we must accept natural selection because it is inconceivable that there should be any other explanation without

assuming the principle of design. Lord Salisbury accepts this dictum and concludes that as natural selection cannot be demonstrated we must accept the principle of design. But surely natural selection and design are not exclusive. Whether or not one species has developed from another through variations (small or large, due or not due to physical environment) and survival of the fittest is a matter which must be decided by observation and experiment. We may judge from the evidence that the doctrine of variation and survival is a correct or an incorrect account of what has taken place. In either case one may or may not believe in a principle of design. J. McK. C.

NEW BOOKS.

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Notes on the Development of a Child. M. W. SHINN. Part II. The University of California, at Berkeley, Cal. Pp. 89–178.

Lehrbuch der allgemeinen Psychologie. J. REHMKE. Hamburg and Leipzig, Leopold Voss, 1894. Pp. 582.

Dolore e piacere: storia naturale dei sentimenti. GIUSEPPE SERGI. Milan, Dumolard, 1894. Pp. xii + 395.

Ueber die Tragwahrnehmungen (Hallucination und Illusion). EDMUND PARISH. Leipzig, Abel, 1894. Pp. 2 + 246.

The Effect of External Influences on Development. A. WEISMANN. Romanes Lecture, trans. by G. Wilson. Oxford, Clarendon Press, 1894.

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Epitome of the Synthetic Philosophy. F. H. COLLINS. With preface by H. SPENCER. Third edition. London, Williams & Norgate, 1894. Pp. xix + 639.

The Æsthetic Element in Morality. F. C. SHARP. New York, Macmillan & Co., 1893. Pp. 131.

NOTES.

Professor HERMANN VON HELMHOLTZ died in Berlin on Sept. 8.

Professor VEITCH, of Glasgow, died in that city in September.

Mr. D. G. RITCHIE, of Oxford, has been appointed to the chair in Philosophy at St. Andrews vacated by Professor JONES.

Mr. W. J. SHAW, B.A., M.A. (Toronto and Princeton), has been appointed Instructor in Philosophy in Wesleyan University.

Mr. S. F. MCLENNAN, B.A. (Toronto), has been appointed Assistant in Psychology in the University of Chicago.

There will be issued yearly, in connection with THE PSYCHOLOGICAL REVIEW, a Bibliography of Psychological Literature, compiled by Dr. Livingston Farrand, Columbia College, and Mr. Howard C. Warren, Princeton University. The bibliography will include, so far as possible, all books, monographs, and articles in psychology, and those publications in philosophy, biology, anthropology, neurology, etc., which are important for psychology. Authors will contribute to the completeness and accuracy of the bibliography by sending to Dr. Farrand or Mr. Warren lists of their publications, with details of title, author, publisher and place of publication (or name of review or archives), and number of pages. The bibliography for 1894 will be issued early in 1895.

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